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I N D E X.

- Absence of bladder, 409.
- Action of chlorate potash, 336.
- Adulterated spirits, Bertheraud on, 92.
- Alum in burns, 42.
- American Medical Association, thirteenth meeting, 68.
- Arago on physicians, 348.
- Aran on diabetes mellitus, 249.
- Army medical reports, 182.
- Army news, 430.
- Army reports (Review), 496.
- Arsenic in apoplexy, 407.
- Artificial milk for babes, 337.
- Asarum in alcoholism, 338.
- Ascites, Croskery on, 384.
- Association seal, 78.

- Brodie, Sir Benjamin, 506.
- Bernard, recurrent sensibility, 207.
- Bernard, experimental pathology, 49.
- Bismuth in burns, 411.
- Boston munificence, 348.
- Brickell, vesico-vaginal fistula, 37.
- Burwell, vaccination, 1.

- Cæsarian operation after death, 489.
- Chew, diabetes mellitus, 93.
- Chloro-anæmia, 407.
- Claiborne's case of stone, 203.
- Climate of the Glades, 168.
- Clinical lectures, Smith's, 177.
- Clinical lectures, Tucker's, 433.
- Compression in varix, 411.
- Constituents of urine, 373.
- Convention medical teachers, 62.
- Convention superintendents of insane hospitals, 86.
- Coolidge, sanitary reports, 182.
- Creosote formula, 412.
- Croup, Gottschalk on, 409.
- Currents and counter-currents in medicine, 255, 423.

- Death of Dr. Luke White, 91.
- Deodorants in midwifery, 492.

- Depaul on occlusion of womb, 484.
- Diabetes mellitus, Chew on, 93.
- Dietetics, Tucker on, 18.
- Digestion, Busch on, 397.
- Diphtheritic paralysis, 300, 345.
- Donaldson on Howard and Manson, 261.
- Durkee on syphilis, 219.
- DuVal's paper noticed, 174.

- Editorial correspondence, 174.
- Effect of music on the sick, 511.
- Effects of lead on the fœtus, 491.
- Errata, 506.
- Erysipelas of infants, 408.
- Ether in deafness, 141.
- Ethnology, Forwood on, 114.
- Examination for navy, 431.
- Extract rhatany, 409.

- Faulkner on jaundice, 355.
- Ferruginous sugar plums, 254.
- Forget on antiphlogistics, 511.
- Forwood, ethnology, 114, 188.
- Freckles of pregnancy, 493.
- Fuqua, excision of clavicle, 358.

- Garibaldi's hospitals, 510.
- Glades, climate of, 168.
- Glycerine poultice, 254.
- Gun-shot wound of penis, 410.

- Hammond's clinical lecture, 349.
- Hecker on os uteri, 147.
- History of iodine, 511.
- Holmes vs. the profession, 255.
- Howard & Manson's controversy, 261.
- Hunter's surgical reports, 136.
- Hunter, post tibial artery, 447.

- Inducements to subscribers, 505.
- Infirmity Medical College, Virginia, reports, 360.
- Iodism, Velpeau on, 55.
- Irritative substitutive medication, 369.

- Jackson on sun's rays, 400.
 Jacobi on diphtheria, 310.
 Johnston's operating chair, 140.

 Laryngoscope, 57.
 Leidy's anatomy, 494.
 Leucorrhœa, treatment of, 413.
 Linnæus' classification of the human race, 511.
 Lithate of quinine, 252.

 McKew on irritative substitution, 472.
 McKew's translations, 369.
 McSherry's obstetrical memoranda, 286.
 Medical Association, 426.
 Medical fees in Turkey, 92.
 Medical appointments, 427.
 Medicine and its deriders, 507.
 Medical College of Virginia, 428.
 Millstone-maker's phthisis, 405.
 Moynier on diphtheria, 363.

 Nightingale's Notes on Nursing, 154.
 Nonat on chlorosis, 461.
 Notes on Nursing, 154.
 N. Y. Med. Society on diphtheria, 340.
 Obstetrical memoranda, McSherry, 286.

 Operating chair, Johnston's, 140.

 Paine's Institutes, 240.
 Pajot on pelvis, 387.
 Pajot on forceps, 482.
 Paralysis of bronchial muscles, 379.
 Pathogenesis of chlorosis, 456.
 Pepsin in vomiting of pregnancy, 413.
 Physiology, Bernard on, 390.
 Physician's Visiting List, 503.
 Pirrie on Eclampsia, 476.
 Potash in bone diseases, 409.
 Pollard on endemic jaundice, 276.

 Roemer on cachexia, 130.
 Roemer, sarco-cirsocele, 443.

 Sanitary convention, 82.
 Santonine, Falck on, 406.
 Sarco-cirsocele, Roemer, 443.
 Sarsaparilla, Sigmund on, 403.
 Scanzoni on convulsions, 486.
 Scirrhus, Hammond on, 349.
 Sedative pills, 408.
 Smith, Professor, on the bladder, 177.
 Smith, amputation of foot, 47.
 Stillé's Therapeutics, 164.
 St. Louis Society on diphtheria, 343.
 Surgical reports, Hunter, 136.
 Sydenham on scarlatina, 511.

 Tetanillus, Trousseau on, 467.
 The association and the colleges, 89.
 The closing year, 504.
 Thirteenth session American Medical Association, 68.
 Transactions of Pennsylvania Medical Society, 415.
 Transfusion in exhaustion, 411.
 Traumatic diabetes, 151.
 Triquet on deafness, 141.
 Trousseau on perchloride of iron, 245.
 Trousseau on tetanillus, 467.
 Trousseau on dysentery, 402.
 Tucker, dietetics, 18.
 Turner on burn, 42.
 Tucker on typhoid fever, 439.

 Vaccination of Indians, 348.
 Vaccination, Burwell on, 1.
 Velpeau on iodism, 55.
 Velpeau, uterine polypi, 149.
 Vesico-vaginal fistula, Brickell, 37.
 Volume xv., 87.

 Warren's introductory, 502.
 West on children, 234.
 Whey and grape cure, 332.

 University of Maryland, 429.
 Urology, Hammond on, 450.
 Urticaria, Scanzoni on, 412.

752

THE

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New Series.

ART. I.—*An Essay on Varioloid.* Read before the Medical Society of Virginia. By BLAIR BURWELL, M. D., Richmond. *Published at the request of the Society.*

MR. PRESIDENT AND GENTLEMEN:—I am here to discharge an obligation imposed upon me by a vote of the Society, and in so doing, if I can but restore order to what is now a labyrinth of confusion, my labor will not have been in vain. I am sensible that life, strength, and glowing colors will be wanting in the execution, but an honest and zealous ambition is my only qualification, commensurate with the importance of the subject.

Small-pox and varioloid have prevailed to a greater extent within the past few years, not only in our own country, but in Europe; and its prevalence has occasioned no slight degree of anxiety in the minds of the profession, as to the protective influence of vaccination. Medical history does not give us any very ancient account of the origin of small-pox, and until very modern times, it was supposed to have had its origin in Egypt about the middle of the sixth century, from whence it spread to Constantinople. But the late writers upon the Sanscrit language have discovered that it existed at a date much anterior

to that time, and that the protective influence of vaccine was also known in India long before it was ever heard of in England. In a Sanscrit work attributed to one Hautuori, there occurs this passage descriptive of their knowledge of the operation and effects of vaccination: "Take the fluid from a pimple, from the teat of a cow or the arm of a man, on the point of a lancet, puncture the arm between the shoulder and elbow, so that the blood appears, and then the fluid mixes with the blood, and the fever of small pox will be the result." He states that the small-pox taken in that way will always be benign, and require no treatment. He also states that this sort of small-pox preserves one from the disease forever. There have of late been other details collected from the same country corroborative of this statement. In 1803, certainly before the news of the discovery in England could have reached Arabia, "the Naw-aub Ali-Khan, seeing his son attacked by small-pox, sent for a Brahmin named Alip Choby, to attend him, who expressed great regret that he had not seen him sooner, as he could have prevented it by inoculating him with the fluid contained in the pustule of the cow."

Mr. William Bruce, Consul at Bushire, wrote to Sir William Erskine in March, 1819, that vaccine had been known for a long time in Persia. Humboldt proves in his "*Essay Politique Sur Le Royaume de la Nouvelle Espagne*," that the inhabitants of the Cordilleras of the Andes, had for a long time noticed the preventive effects of vaccine. He says that a negro who had been unsuccessfully inoculated, refused to submit again to the operation, because (he said) that he had contracted in milking the cows of the Andes, the sort of eruption seen on their teats, which prevented one from having the small-pox. Here was a tolerable perfect knowledge of small-pox and vaccination existing and in practice centuries ago, but it was impossible that it could have been known in England before Jenner's discovery. He could not have had any knowledge of it, and neither Bruce nor Humboldt have sought to detract one iota from the glory of his discovery. It is now a little more than sixty years, since Dr. Jenner (a man not unknown to the scien-

tific world) announced his discovery of vaccination—a means whereby mankind might for the future be relieved of the terrors of that most loathsome disease—the small-pox. This announcement, although it was received with some distrust, yet it excited, in a remarkable degree, public attention, for it was not the impulse of a youthful aspirant to fame, but the calm result of much thought and deliberation, matured during a period of more than twenty years. The early career of vaccination, however, was not without its drawbacks. There were a few who opposed it with a malignant animosity, which carried with it great professional jealousy, and a regular systematic attempt was made to wrest from Jenner's brow the laurels he had so fairly won. It is true, that there were those who had heard and known the common rumor that the milk-maids of Gloucestershire were free from the ravages of this much dreaded disease, by virtue of the cow-pox, but it had never entered their minds that there was anything of an universal practical benefit in that. It is true, that there were those who, like the disciples of Aristotle, had gazed upon the beauties of a cloudless sky for many a night, without perceiving the movements of Heaven's machinery until Galileo pointed it out. It is true, they had seen the apple fall to the ground, but they had never thought of gravitation until Newton announced it. It is true, that they had heard and witnessed the roar and flashing of Heaven's artillery for centuries past, but to Franklin and to Morse the honor was reserved of turning it to practical use. So was reserved to Jenner the protection of mankind from the ravages of small-pox.

It is this very month, sixty-two years ago, that Jenner first published his work upon the subject of cow-pox, and soon his name and fame were heralded throughout the length and breadth of the civilized world. Jenner and many of his ardent admirers fondly hoped, that by the general adoption of vaccination, the disease, small-pox, would be erased from the catalogue of diseases; and, perhaps, stimulated by the plaudits of a grateful people for so glorious an achievement, he too hastily stated that “the constitution once fairly subjected to

the influence of vaccine virus, it would remain through life insusceptible of variolous contagion."

This announcement, at the time, added prodigiously to the brilliant splendor of Jenner's fame; but perhaps it would have been better for him and mankind also, if he had not gone so far. It certainly was premature to announce such a belief in the early days of vaccination, as the truth of the position could only be ascertained by the results of time and experience. The frequent occurrence of small-pox and varioloid since 1818 has unfortunately disproved the correctness of his anticipations, and the apparent steady increase of these diseases within the past few years, both in Europe and America, has created no little distrust in the public mind as to its protective value, and the consequence of this distrust has been to unsettle all preconceived opinions upon this subject. It becomes, therefore, a most serious enquiry whether, from all the information of the past sixty-two years, the present state of vaccination is such as to justify distrust in its efficacy as a preventive of variolous disease. In order to arrive at a just and fair conclusion of this important question, it will be proper (I conceive) to consider thoroughly the following points, viz:

1st. Is the protective power of vaccination (or cow-pox) absolute, or merely temporary?

2d. If it is temporary, does re-vaccination add to, or renew the security against the small-pox or varioloid?

3d. If it does add security from small-pox, for what period of time does it exert the wished-for protection?

4th. Has vaccine virus taken directly from the cow, a more certain and durable protective power than vaccine matter transmitted a greater or less number of times through the human subject? In other words, has the vaccine virus of the present day lost any of its protective power from having been repeatedly transmitted from one human being to another?

5th. If the vaccine virus has become enfeebled in its protective power, can it be renewed, and how?

These five points (I believe) embrace all that is necessary; if answered satisfactorily, to give a full, fair and proper under-

standing of the state of vaccination. Let us see if they can be so answered.

1st. Is the protective power of cow-pox or vaccination absolute or temporary? What is cow-pox? It is now generally believed by most professional men (everywhere) that cow-pox is small-pox in a milder form, having been modified by passing through the system of the cow. Now, does not the experience of the world prove that the protective power of the small-pox itself is not absolute against a second attack of the same disease? Then, if the small-pox, the more powerful virus, is not absolute against itself, is it to be expected that the cow-pox, the weaker virus, will be more potent and absolute against the stronger virus, the small-pox? I say no. Common sense, as well as experience, proves the negative. But there are those who will deny that the small-pox is not a sure preventive against a second attack; or, in other words, they say that the protective power of small-pox is absolute against a second attack; that the few cases of a second attack of small-pox are the exceptions to prove the rule. In reply, I would say, examine the record; and they will find that the exceptions are too numerous; they prove the negative of the rule. I am aware that there is some difficulty in furnishing statistical evidence upon this point; but, reasoning by analogy, I think it is very clear. Small-pox after small-pox, it is true, is much less common than small-pox after cow-pox; but it is because of the comparative rarity, now-a-days, of primary small-pox to the number of vaccinated persons. But to the proof by vaccination.

It is agreed, on all hands, that if a person can take the small-pox, they could take the cow-pox (if engrafted) and vice versa. Now let us see what proportion vaccination (or cow-pox) after small-pox bears to re-vaccination (or vaccination after cow-pox). The army statistics upon this point furnish numerous and conclusive data. In the Hanoverian army, in 1837, '38 and '39, experiments were performed of this kind; 100 men who had been vaccinated a long time, and had well marked scars on their arms, and a like number of men who had had the small-pox (and bore well marked pits on their faces) were

vaccinated at the same time, and the results of successful operations (or vaccinations) were just about equal in the two classes. Dr. Heim, of Wurtemberg, performed exactly the same experiment, and he says that he found of those who had been re-vaccinated, the operation succeeded perfectly in 34, produced a modified effect in 25, and no effect at all in 41. Of those who had the small-pox, the vaccine succeeded perfectly in 32, produced a modified effect in 26, and no effect in 42. Dr. Stark, of England, noticed the same proportionate results as Dr. Heim, and many others have observed the same thing. There is an instance of the equal susceptibility to small-pox by those who have had the small-pox, and those who have taken the cow-pox, found in the report of one of the orphan asylums of England. Of 5,774 boys admitted into the institution during a period of years, I find 1,950 bore the marks of small-pox; 3,636 bore the marks of vaccination, and 188 had no mark of either. Now the last were vaccinated on their admission into the asylum; so that there were 1,950 who had marks of small-pox and 3,824 who had been vaccinated. Now, of those who had had the small-pox, 12 had it again, about 6 to 1,000. Of the vaccinated ones, 27 had the small-pox, about 7 to 1,000. Thus showing that in the proportion of persons who have had the small-pox, to persons vaccinated, *ceteris paribus*, they are alike, and equally subject to small-pox.

Again, Dr. George Hayward, in a communication to the Boston Medical Journal, says: "The opinion that the protective power of vaccine becomes exhausted and worn out in time, does not appear to be well founded. It," he says, "is contrary to analogy, and rests on no well authenticated facts." This I deny; for in the only analogous instance, it is proved, beyond doubt, that the influence of small-pox (both natural and artificial) and varioloid are only protective for a time. It is now conceded by nearly all the Medical Boards and Societies of Europe, that the influence of vaccine is temporary in a majority of instances, if not in all; and thus re-vaccination is recommended and generally practiced. But, says Dr. Hayward, a statement of Dr. Adams is quite decisive upon this

point. He re-vaccinated 490 persons, and found that the least susceptibility seemed actually to have existed among those whose vaccination was the most remote. Now what is this single instance of Dr. Adams' experience, compared with MM. Teuffard, Breschet, D'Espine, Holland, Simon, and others, all of whom agree with the general evidence furnished by the army statistics, that the very opposite in their experience is true, viz., "that the re-susceptibility to vaccine is greater in proportion to the time which has elapsed from the first vaccination." Again, the authors of all the memoirs presented to the French Academy in 1845, unanimously agreed that, with very few exceptions, the small-pox attacks those who have been vaccinated for a long period of time and spares those who had been recently so. If all this evidence is not sufficient, I will farther add the opinions of Magendie, Roux, Larrey, Savart, Blanville, Bosquet, Heim, Stark, and Gregory, who not only believe that the protective power of vaccine is only temporary, but they express their belief that it is not safe to trust to it longer than fifteen years, without renewing its protective influence. Now, it would seem with such proof before us, if cow-pox is modified small-pox, and if one attack of small-pox itself does not render an individual absolutely insusceptible of variolous contagion, which is proved by actual experience, and also by the test of vaccination after small-pox, that we should not by any means expect vaccination, or cow-pox, to be absolute protection against small-pox. I now pass to the second point.

2d. If it is temporary, (which I think is sufficiently proved,) does re-vaccination add to or renew the security against small-pox and varioloid? It is admitted by every one, that vaccination is a preventive of variola, for a limited period at least, and *that being so*, if the first vaccination is wearing out and its influence becoming exhausted, it is very natural to expect that if you engraft the vaccine a second and even a third time, that the system will renew its resistance to variola, just in proportion to the development of the pustule, and the pustule will develop* itself in proportion to the re-susceptibility of the system. But there are a sufficient number of practical evidences

of this fact. Mr. Simon, in his report to the German Board of Health, showed that for a period of five years, commencing with 1833, of 14,384 re-vaccinated soldiers of the Wurtemberg army, only a single case of the disease occurred, (and that was varioloid,) although the small-pox was introduced into the regiments on sixteen occasions during the same time. The Prussian army furnishes still more conclusive testimony in favor of re-vaccination than that of Wurtemberg. For ten years preceding 1831, small-pox and varioloid increased regularly every year, although every soldier bore the marks of vaccination, so that from 1831 to 1833 there were no less than 312 deaths from small-pox. For the twenty years succeeding, the Prussian army represented an entire re-vaccinated population, and there has only been an average of two deaths annually since, of small-pox. In a report made to the government by the Physician-General of the Prussian army in 1834-5, there were 48,478 soldiers re-vaccinated. The operation was successful in 15,269; partially successful in 12,203, and it had no effect in 21,006; those who were not affected by the first re-vaccination were again re-vaccinated, and it was perfectly successful in 784 cases. Of that large number (in the two years), only fifty had varioloid and twenty, small-pox, and it is not stated whether those belonged to the class that were not successful, or otherwise. The English reports furnish, also, the strongest evidence of the renewed efficacy of re-vaccination. Thus Dr. Mairston, physician to the small-pox and vaccination hospital of London, states that not one of the nurses or servants of the hospital has had small-pox for the last twenty years, and he attributes it to the fact *that they are either vaccinated or re-vaccinated at the time they come to live in the hospital*. A very striking example of the protective power of re-vaccination occurred in this city, not long ago, in the family of one of our brother physicians. (I know he will pardon me for alluding to it.) Having the small-pox on an adjoining lot to his, he re-vaccinated every member of his family, white and black, except an old lady, who thought she was not susceptible to the variola, and two of the servants, who happened not to

be near at hand at the time, but both had previously been vaccinated. The result was, that none of his family took small-pox or varioloid, except the three who were not vaccinated, and all three of those had severe cases of varioloid; the lady died; the two servants recovered.* This point of the subject, gentlemen, you all have had an opportunity of verifying in the late epidemic of small-pox and varioloid in this city. You have seen the disease, one or the other, attacking almost alone those who have not been vaccinated, or re-vaccinated for a period of years, and you have most generally been able to confine it to a single member of a family *by the protective power of re-vaccination*.

In the "Dictionaire de Medecin," published as late as 1847, where the subject of re-vaccination has been thoroughly discussed, I find that in France, Sweden, Denmark, Germany and Italy, the profession almost unanimously hold the opinion that the protective power of vaccine is only temporary, and that the only safety from the ravages of epidemic small-pox is in the regular performance of re-vaccination at intervals of time varying from ten to fifteen years. These opinions are also entertained by a majority of the English profession of the present day.

Having arrived at the conclusion that re-vaccination does renew the security against the small-pox and varioloid, the question now is for what period of time does it afford protection, and how often ought it to be renewed in the same individual? This is one of the most important points connected with the whole subject of re-vaccination, and the one in which it is most difficult to arrive at any definite and accurate conclusion. It is a problem the solution of which would require a general investigation into every case in which vaccination has been performed. From my own observation and reflection, the period cannot be fixed at any positive and definite period; for I believe it varies with different individuals according to the differ-

* Since writing the above, I have ascertained that this was a case of unmitigated small-pox, and that the lady was one among the first persons vaccinated in this country. She was vaccinated by Dr. Spence of Dumfries, Va., who obtained the matter from Dr. Jenner himself.

ence of constitution, and I also believe the period of immunity is much less than most persons suppose. In the cases of varioloid and small-pox which I have observed, they have never occurred until after the tenth year from vaccination, and the re-vaccinations which I have performed have invariably been unsuccessful previous to the seventh or eighth year from the first vaccination, and after the tenth year a large number have been successful, with a good many cases partially successful, and *very, very few* in which no impression has been produced. By a reference to my note-book, you will find that from the 3d day of November, 1859, to the 16th day of April, 1860, I vaccinated thirty-seven persons. Eight of those were infants, all of which were successful; but three of them were repeated twice before succeeding, and one three times. Twenty-nine of the thirty-seven were re-vaccinations; that is, they were on persons who had been vaccinated before, and nearly every one of them bore the marks of good vaccine scars on their arms. Twelve of the twenty-nine were successful re-vaccinations; of the twelve successful ones, seven were in persons who had been vaccinated for fifteen years and upwards, four were in persons who had been vaccinated between ten and fifteen years, and one had been vaccinated between five and ten years. There were ten of the twenty-nine partially successful; of the ten partially successful ones, six were persons who had been vaccinated between seven and ten years; three were persons who had been vaccinated between ten and fifteen years, and one had not been vaccinated for more than fifteen years. Of the remaining of the twenty-nine, there were seven who were not affected at all. Two of the seven were persons who had been vaccinated within less than five years; three of them were persons who had been vaccinated between five and ten years, and the other two had been vaccinated more than fifteen years. (I would remark that several of these persons were vaccinated one, two and even three times before there was any result.)

During the above dates, I also attended two cases of varioloid, (both in young ladies not twenty years old,) and both had good vaccine scars on their arms—both recollected their first

vaccinations well, and they stated that it was between ten and fifteen years since they were first vaccinated. From the experience of these twenty-nine re-vaccinations, and the two cases of varioloid, it appears that the protective influence of vaccine extended for a period of years varying from ten to fifteen. Then, may we not be safe in recommending a regular time for renewal midway between these two periods—say twelve to thirteen years, unless epidemic disease should require it oftener, and then we might adopt ten years.

Upon this point, though, let us not be content with so limited an experience, but let us see what information “statistical Europe” can furnish. In a report made to the general government of France by the Royal Academy of Medicine in 1832, I find the experience of Monsieur Tueffard, a gentleman of known ability and research, upon this subject detailed. Monsieur Tueffard re-vaccinated 236 individuals. These he divided into four classes, as follows: 1st class consisted of 108 individuals, who were re-vaccinated in about a year after the first vaccination. Of this 108 there were only five who presented any effect of the re-vaccination, and those only in the very slightest degree. The 2d class consisted of 20 individuals, in whom the period of re vaccination extended from one to seven years after the original operation. Of these 20, there were seven affected with incomplete secondary vaccine—the other 13 were not affected at all. The 3rd class embraced 53 individuals, who were re-vaccinated from seven to fifteen years after the original vaccination. Of these 53, there were 12 who had a genuine vaccine pustule, 25 took the vaccine imperfectly, and sixteen were not affected at all. The 4th class comprised 55 persons, in whom the interval between the vaccination and re-vaccination exceeded fifteen years. Of these 55, there were presented 16 cases of perfect vaccine pustule, 20 of incomplete, and 19 that were not affected. Now a comparative examination of the above report shows conclusively that protection is almost, if not quite, complete up to the seventh year after vaccination; but, after that period, the re-susceptibility to cow-pox (and consequently to small-pox

and varioloid) increases in a direct ratio with the length of time from the date of vaccination. In England, Dr. Holland, speaking of re-vaccination, says: "It becomes of consequence to define the best period for a practice unequivocally desirable at one time or another, and reviewing all that has yet been learned upon the subject, I am led to believe, that where there is a fair proof of the original vaccination being perfect in kind, the period best to be selected for repetition is that from ten to twelve years." (Med. Chi. Rev.) In the 'Medical Times & Gazette' for December, 1859, in the instructions to the public vaccinators by the Lords of Her Majesty's Most Honorable Privy Council, we find directions showing what is the prevailing opinion of that honorable body upon this point; which corresponds nearly with those already cited. The instructions are, not to re-vaccinate persons who have been efficiently vaccinated in infancy before the fifteenth year, unless there be immediate danger of small-pox contagion; in which instance they are directed to afford *that protection* to those of twelve years of age. In the same number of the journal there is a very interesting account of a fearful epidemic of small-pox and varioloid, which visited the Canton of Geneva in 1858-9, by M. D'Espine. After giving an account of the epidemic as the most fearful that had occurred since the introduction of vaccine, Dr. D'Espine says: "Re-vaccination made at opportune periods, greatly increases the chances of preservation, and evidently alleviates the disease in those individuals whom it has not been able to secure against the attack. He adds farther, that re-vaccination seldom succeeds in producing satisfactory pustules until after ten years of age. He says on this account then, and because the first vaccination affords sufficient protection, at least until ten years, it is at about from the twelfth to the fifteenth year that the first re-vaccination should be performed. A second re-vaccination, he says, may be resorted to about the age of thirty years, but this is of less consequence, as an examination of facts show that the chances of contracting variola diminish much after the age of thirty. I might offer farther evidence upon this point, but

I think if you will only sum up all the facts which I have here presented, you will be satisfied that the period for re-vaccination is between the tenth and fifteenth years, for at least nine-tenths of those who have previously been vaccinated; and as a small proportion of the vaccinated are susceptible to the influence of cow-pox (and consequently small-pox) about the tenth year after vaccination, and as no harm can be done to an individual who is not susceptible to genuine or complete cow-pox by re-vaccination, I think it wise to recommend the minimum period of re-susceptibility as the safest period for general renewal of vaccination—and that period is about ten years.

Having thus disposed of the question of the proper time for the renewal of vaccination, I pass on to the fourth question, which is, perhaps, the most important point concerning the whole subject of vaccination; as upon it depends the value of much that is done, and every thing in the future of protection from the ravages of small-pox. The question is: "Has vaccine virus, taken directly from the cow, a more certain and durable protective power than vaccine matter, transmitted a greater or less number of times through the human subject; or, in other words, has the vaccine virus of the present day lost any of its protective power from having been repeatedly transmitted from one human being to another?" I am aware that this question has been a good deal discussed, and that some very high authority has maintained and still defends the negative; but I think I will be able to show, that if there is not positive proof of the affirmative of this question, there is at least very strong reasons for suspicion.

In the first place, if vaccine has not become enfeebled in its powers, I would ask why is it that there has been a greater increase in the number of cases of small-pox and varioloid after vaccination, proportionally, than was formerly known, and why has the period of protection become shorter? It is a well known fact, that up to 1818 the cases of small-pox after vaccination, (or varioloid) were so few that they were not generally believed, and the persons reporting them were

thought by every body to be mistaken ; but, about 1825, they became so numerous that the disease 'varioid' was well known. Again: The number of unsuccessful vaccinations have very much increased of late years. Both in Europe and America, thirty and forty years ago, it certainly was very uncommon to fail in more than two or three per cent. of the vaccinations, whereas now we often fail in fifteen to twenty, and sometimes twenty-five per cent. M. Bosquet, in 1839, tells us that he had the same happy success with new vaccine lymph which he was enabled to obtain directly from the cow. He says, that he used the new vaccine lymph in several hundred cases of vaccination and scarcely failed more than once, whereas he failed very often with that which was in common use. In re-vaccinations, also, he says he succeeded frequently in individuals with the new lymph, whom he could not affect with the old in use. Now why is this, if the power of the vaccine virus is the same? Will we of the present day admit that we do not take as much pains as our forefathers, or that they possessed a more "tactus eruditus" than we? No; rather ascribe it to its true cause, the *degeneration of vaccine matter*.

Again: It has been observed by many of the public vaccinators of Europe, that there is a marked difference in the appearance of the old scars and those of late years. Meyer, as early as 1825, noticed the difference in the appearance of the cicatrices. In an examination of over four thousand persons, he said the elder ones were much more marked; and he also stated that the scars from his own vaccinations with lymph directly from the cow were of the old normal type. Drs. Gregory and Estlin, of England, have both noticed similar facts as evidence, that the vaccine lymph, in passing through the systems of a great many persons, loses some essential part of its activity. The celebrated Dr. Watson says: "It appears that where the vaccine supply has seldom or never been renewed from the cow, the proportionate re-susceptibility of vaccine disease at a given age (and therefore, it may fairly be presumed, the susceptibility of small-pox also) has undergone a progressive increase." "Therefore," argues Mr. Simon, "just

as post vaccinal small-pox has increased, it is difficult to conceive how the infantine generations of a country could, crop by crop, successively derive less permanent constitutional impressions from vaccination, unless the efficient cause of those impressions, (the vaccine contagion itself), had year by year undergone enfeeblement of its powers.

Upon this point the reports of the Prussian army, from 1833 (the period at which general vaccination commenced) to 1857, furnished the most undoubted evidence. The vaccination of its recruits (who are subjects of like age, in like proportions, and under like discipline and treatment) number annually some forty or forty-five thousand operations. When the system of re-vaccination was commenced, it was found that about thirty-three in every hundred were successful. Now, the annual percentages of successful results for the whole period of twenty-four years, increases very regularly up to 1857, when we find the percentage for that year standing at seventy, and for the three years preceding, double that with which the series commenced.

Now, the defenders of the non-depreciation of vaccine virus themselves admit, that of late years small-pox and varioloid have been on the increase; they admit that the number of successful first vaccinations have been less in the last twenty-five and thirty years; they admit that the proportion of successful re vaccinations is on the increase; they are forced to admit that there is marked difference in the appearance of the cicatrix of the old and new vaccination; they are forced to admit that vaccine matter, taken directly from the cow, causes local symptoms of greater intensity, and that its effects are also more certain than that of the old vaccine matter; and yet they say that they do not believe the vaccine virus has undergone any change or lost any of its protective power. They admit every point and fact in the train of reasoning, to prove the affirmative, and yet they deny the conclusion, and assert the negative. They are forced to admit it, or they must acknowledge that we of the present day are not so skillful and careful as our forefathers. Which of the two will ye choose?

This, gentlemen, brings me to the fifth and last point of discussion upon this question, and that is: If the vaccine virus has become enfeebled in its protective power, can it be renewed, and how? All agree that it can be renewed from the same source which Jenner obtained it, (which is certainly very difficult to find,) but there is considerable difficulty, if not great doubt, as to whether the disease can be communicated to the cow at pleasure. Dr. Jenner considered that vaccine disease had its parent in the horse, and that it was communicated to the cow. Robert, of Marseilles, thought that it was the result of small-pox inoculated in the cow; while others think it is a disease peculiar to the cow. Each of these opinions have had their advocates; but now the generally conceded opinion is that "it is small-pox in the cow." A great many have experimented with small-pox virus upon the cow. But few of them have succeeded; among them I will mention Messrs. Simon, Gassner, Thiele, Ceely, Badcock, Sonderland and a few others; while Newman, and Macpherson (of England) and Cutter, Martin and others of America, have entirely failed. In the Boston Medical Journal for February (1860) there is detailed the experiment of a Dr. Martin (of Attleboro', Massachusetts), in 1836, who attempted to obtain vaccine virus, by engrafting small-pox matter upon the cow; the result was that he obtained a virus that communicated variola to every person that he vaccinated, or rather "*inoculated*," with it. But there are a number of well authenticated instances on record, where vaccine matter has been obtained by inoculating the cow with small-pox virus; and the only explanation of the failure to produce a benign vaccine virus at all times, must be found (I think) in our want of knowledge as to time and condition of system of the cow, for its proper production. There are a number of medical men who declare that vaccine virus is regenerated by repassing it through the system of the cow; while, on the other hand, there are an equal number who declare that the virus is not at all altered or improved by passing it through the system of the cow. When doctors disagree who shall decide? After examining the whole subject thoroughly, con-

sidering everything that has been written and said on the means proposed for the renewal of the vaccine virus, the Royal Academy of France, with all their learning, research and sound judgment, have declared that "the only means that science can have any confidence in at the present day (for the renewal of the vaccine virus), is to obtain it from its original source, as found in the cow."

This, gentlemen, brings the subject to a close for the present. You have now my views at full length, after having examined this subject as thoroughly and as fully as my means and ability would permit. I hope I have at least succeeded in influencing your minds in the right direction; but, be that as it may, I will express the wish that some of you will consider the same subject at another time, and should you succeed in proving my position to be wrong, I shall not be sorry, for the truth is all we desire; but until you do, or there are farther reasons presented than have yet been given, I shall continue to believe that "the protective power of vaccine is temporary; that re-vaccination does add or renew security against the small-pox and varioloid; that the period of time for which it exerts its protection is between ten and fifteen years; that it ought to be renewed regularly between those periods; that the vaccine virus of the present day has degenerated, or become weaker in its protective powers; and, lastly, that a resort to the original source is the proper means to restore it."

ART. II.—*Lectures on Dietetics*.* By ALFRED B. TUCKER, M. D., of Clarke county.

THERE is probably no subject, upon which the young practitioner finds himself so practically deficient, as a proper regulation of diet in the treatment of disease. Without such regulation, the articles of the *materia medica* may be tried in vain, while with it, Nature herself, thus aided, will frequently perform the cure.

Let us enquire first, the effects of food when taken into the system, as well as the necessity for it.

Without entering into the process of digestion, which belongs more naturally to physiology, it is only necessary to know that the food in its passage through the alimentary canal undergoes certain changes, differing in different kinds of food, and that these changes being produced, such portions are absorbed, as the support of the system requires, and those portions not so required, are carried off by the different depurating organs of the body.

The first effect of digested food upon the system, is the nutrition of the body.

There is a continual change going on in the system. Each organ of the body is constantly undergoing change, *e. g.*, the secreting glands throw off the cells of which they are composed, and upon which their constitution depends. Now, to provide against this waste and compensate therefor, it is necessary that some arrangement should be made. This is accomplished by the food taken into the body, which, after undergoing the changes consequent upon digestion, is absorbed by the lacteals and veins, and becomes a component part of the blood; and from its passage through the system, nutrition of the body results, the epithelial cells of the various secreting and excre-

* The following lectures, in substance, were delivered in the Winchester (Va.) Medical College, several years since, as a part of a regular course upon *Materia Medica*. No claim for originality is made, but they are simply committed to print as a compilation of views at the present date.

ting organs, taking such portions as are necessary for their own repair.

This is simple nutrition, a process whereby a proper relation is kept up between decay and reproduction. In some cases of disease, however, the waste of the system is greater than natural, and the powers of digestion not being proportionably increased, emaciation, the extent and rapidity of which is governed by a want of balance between the two processes, is the result. And this emaciation will result, when the reverse is the case, the powers of digestion being enfeebled and the usual amount of waste going on in the system.

There is a condition, however, the result of no diseased action, in which, owing to the changes going on in youth, the balance is lost; for example, when a child is growing more or less rapidly, the powers of digestion not only have to replace the ordinary and natural waste of the system, but also provide something for the growth of the tissues: another proof that we must look for the rationale of symptoms, before we pronounce an effect the result of diseased action. If digestion increases in proportion to the demands made upon it, the balance is still maintained, but if such is not the case, a proper and adequate nutrition of the tissues is lost, giving rise to one or the other of those cachexiæ or diseases caused by defective nutrition; hence the development of struma and tubercles, during childhood and at puberty.

The necessity for food. If what has been said above be correct, the necessity for food is exactly in proportion to the amount of waste. Now this decay of the system is governed by the amount of labor performed; we find that a blacksmith or laboring man of any sort eats more heartily and digests better than our city loafer, who considers it degrading to gain his bread by the sweat of his brow; and the reason is, that the amount of labor performed by a laboring man, produces more waste in the system, than when both mind and body are in complete repose. Every contraction of a muscle, every exercise of nervous power produces a certain amount of waste which must be compensated for. Just in proportion, therefore,

as you have muscular action, do you have muscular waste, and a necessity for muscular reparation.

Nor is this waste confined to mere physical exercise. The intense application of the student gives rise to as much decay, as the strong blows of a smith at his forge; the decay, however, being confined to different elements, in the two cases, and demanding different kinds of nourishment for their reparation.

We call your attention to a distinction we have endeavored to draw, between the amount of food taken and the quantity digested. You remember, that of the food taken into the stomach, that which is fit for nutrition is taken up by the blood, and the unfit is carried off through the excreting organs. If, therefore, a large amount of food be taken, containing a small amount of digestible matter, it has no more effect upon nutrition, than if a small quantity of food were taken, containing the same amount of material for repair. And if, from disease of the digestive organs, the food, even if the proper and most digestible were taken, could not be properly acted upon by the intestinal fluids and would not therefore be adequate to the end desired. Bearing in mind, then, that amount of nutrition depends upon the quantity of aliment digested, we are not liable to have any confusion upon the subject.

We conclude, then, that the chief effect of food is to nourish the body, and that this nutrition depends on the amount digested.

Different kinds of food. It will be obvious to any one who will think a moment, that the different tissues of the body require different kinds of food; for while their ultimate components may be the same, their proximate principles differ widely. Were there any doubt of this fact, the proof could be adduced in numberless instances, that an animal deprived of one kind of food will not thrive or even live. It is proper, therefore, to know the kind of food necessary for the different tissues, and, of course, the constitution both of food and tissue. This carried to its highest point is really the province of chemistry and chemical physiology, but to pursue our subject pro-

perly, it is necessary to advert to the facts, which we find made ready to our hands by chemists and physiologists.

The food is of two kinds, viz., nitrogenized and that containing no nitrogen: the former has been divided into albuminous and gelatinous, and the latter into saccharine and oleaginous, but the great similarity between gelatin and albumen has induced many to discard the gelatinous; to which Prout has added the *aqueous*, because the food is mostly presented dissolved in its own water, and water frequently enters into the transformations undergone in the system.

Now man is an omnivorous animal, and cannot be sustained in complete health by a diet either exclusively vegetable or animal. The food furnished by animals is principally nitrogenous, consisting of albumen, fibrine, caseine, &c., all of which goes to the formation of blood, muscle, &c. Now, as both classes of food are necessary, it follows that health will not be preserved by a strictly animal diet. It is true, that the fat of animal tissues does not contain nitrogen; but for some reason not yet satisfactorily accounted for, the azotised vegetable food will not be a substitute for animal, nor the non-azotised animal for non-azotised vegetable.

Vegetable substances, however, contain both kinds of food, as is shown by the ox, which being entirely herbiferous, yet has the nitrogenous tissues nourished in a high degree. It would seem, therefore, that man would live longer upon an exclusively vegetable diet than upon that consisting only of animal matter.

Direct experiment, however, has shown that neither can, for any length of time, sustain life, when used to the exclusion of the other. In, therefore, choosing a diet for a person in health, we must have in view a proper regulation of the quantities of each. This result, however, will be greatly modified by climate, the inhabitants of a cold climate demanding more of the non-azotised, while those of a warmer climate require the azotised—the reason of which difference will now be explained.

Without stopping to enter fully into the question, it is enough

to state that Todd and Bowman propose to give the name of *nutritive* to the nitrogenous compounds, and *calorific* to the non-nitrogenous, basing their division upon the fact, that while the former contribute principally to nutrition, the latter maintains animal heat. This latter, which was long thought to be due to respiration, they hold to be caused by the oxidation of the carbon which prevails in the non-azotised compounds, depending merely upon the fact that chemical union always gives rise to heat. This idea is sustained by the treatment which we receive at the hand of nature. In infancy and early childhood, when the calorific power is low, nature has provided us with those articles of food which contain abundantly the non-azotised materials, e. g. milk, and in after years, when mental and physical labor call for greater nutrition of our bodies, she supplies us with nitrogenous compounds well adapted to the end.

Before concluding the physiology of dietetics, it is necessary to notice the effects of different kinds of food upon the secretions and tissues of the body. We simply give the results, without entering elaborately into the experiments or causes which have led to these conclusions.

Nitrogenized articles produce an increased quantity of the *red corpuscles* of the blood. They also increase, to some extent, the *fibrine*.

Animal diet increases the *urea* of the urine; vegetable food diminishes it. Carpenter (Prin. Human Phys.) says neither have any effect on *uric acid*. Kirke and Paget dissent, saying that the effect is the same as upon *urea*. When we remember that this is the acid upon which gout is supposed to depend, we are constrained to adopt the latter view.

Oleaginous diet increases the action of the liver, and consequently the quantity of the *bile*.

Vegetable diet being the only kind of food containing saccharine matter, increases the amount of sugar in the urine, while animal diet diminishes it.

If we have been successful in establishing the points we have attempted to make, it will be easy to apply these principles to the treatment of diseases. But as there would be fewer cases

of disease, if a proper diet was adopted in health, we call your attention to this point :

The food, as a general rule, must consist both of the nitrogenous and non-azotised elements. Were there any doubt of this proposition, it would be speedily dissipated both by analogy and every-day experience. Upon an analysis of the food provided by nature for man and all animals, we find it to contain both kinds of aliment distributed in such proportions as are necessary for the best support of the animal: for example, in milk we find the albuminous represented by the *caseine*, the oleaginous by the butter, the aqueous by the water of milk, and the saccharine by the sugar of milk. This is true; also, of the egg, which is for the nourishment of the embryo, and which contains all of these elements within it. At a time, therefore, when an animal, even human, is incapable of judging of the substances best suited to his health, nature has provided him with that which is pleasant to his taste, and combines all that is necessary for his support.

The same constituents enter into the composition of wheat bread, which has always been regarded as the “staff of life.”

It is scarcely necessary to advert to the experimental proof of the proposition. The mother, whose diet is merely vegetable and trashy, will entail on her suckling, a pale and languid face, with diarrhœa, cholera infantum, &c., following in the train. The pale face of the young girl, who, fearful of being too fat, will eat nothing containing oleaginous matters, shows the result, and in deranged health she receives the proper penalty of her folly.

There is one point connected with this fact, to which my attention was first called by an article of Dr. Charles Hooker, of Connecticut, on “Diet of the Sick,” in the Transactions of the American Medical Association, vol. viii. It is the connection between the cachectic and a distaste and a non-use of oleaginous matters. How far it is true we cannot say; but of one thing there is no doubt, that it does have a considerable effect, if not as a cure, at least as a prophylactic. The

inhabitants of *Iceland*, who, by their habits and occupations, would be supposed, of all others, to be prone to struma, are entirely free therefrom; and when we remember that their diet is chiefly of the fat and oil, it shows a curious coincidence, if nothing else. Cod-liver oil, too, once supposed to have great medical virtues in the cure of phthisis and scrofula, is now acknowledged to have no remedial properties beyond the oily constituents.

Dr. Hooker gives the following as his results on the subject:—

“1. Of all persons between the ages of fifteen and twenty-one years, more than one-fifth eat no fat meat.

“2. Of persons at the age of forty-five, all, excepting less than one in fifty, habitually use fat meat.

“3. Of persons who avoid fat meat, between the ages of fifteen and twenty-two, a few acquire an appetite for it, and live to a good old age, while the great proportion die, before forty-five, of phthisis.

“4. Of persons dying with phthisis, between the ages of fifteen and forty-five, nine-tenths at least have never used fat meat.”

It is not in my power to disprove the strong ground thus taken by Dr. H., but it would require a long experience and great observation to prove the truth of his conclusions. For, when we consider the subject, it is hard to determine whether the development of phthisis is a *post* or *proper hoc* of the abstinence from fat meat or oleaginous materials: we know that phthisis is very apt to be developed in persons in whom the vital powers are depressed, and this depression is generally found in those who abstain from a strong, healthy diet; but whether the phthisis is developed because they eat no fat, or whether they eat no fat because phthisis has a strong tendency in their system, I cannot answer; but it is true, that fatty matters and a liberal diet are the only chances for phthisis pulmonalis.

The quantity and quality of drink necessary in health is a point of much importance. When we consider, theoretically, the immense evil that may result from a dilution of the secretions

of the alimentary canal, we are not surprised at the deleterious results practically. The practice of drinking large quantities of water during a meal, and at other times, is very common, and as injurious as common: the gastric juice is diluted; all of the juices are unable to perform their functions properly; the blood is rendered so much thinner than usual, as to escape from the vessels in the form of purpura hemorrhagica or dropsy, and the kidneys have an undue tax upon their powers, resulting in some renal or urinary disease—though a certain quantity of water is necessary to promote digestion.

With regard to the use of alcoholic stimulants, we have only to say, that, in case of sickness, they are sometimes useful; but for a person in health to indulge in them is injurious, beyond a doubt, to health; we do not now speak of the moral injury which may result. But we consider that man, as he first came from the hands of God, was perfect in all his physical proportions; if he was not, if the heart, lungs, stomach and brain could not rightly perform their functions in health without the stimulus of alcohol, then it was no Divine hand that fashioned and pronounced all his workmanship good; as therefore we cannot doubt the Divinity, we must believe that man by nature needs no stimulus; and what he does not need must be hurtful to his constitution. This only applies to health; for we gladly sanction its use in disease as invaluable.

It is not necessary to mention regularity in the hours of eating and strict attention to a proper mastication of the food.

In regard to the form in which food is most digestible, we believe that, as a general rule, it is more so solid, than as a soup; the latter, by its watery solution, distending the stomach, without adequate nourishment, and better rare, than done too much.

The mode in which food is cooked has much effect, and we believe that that which is roasted is in a more digestible condition than any other. Attempts have been made to have tables of the relative digestibility of different kinds of food, but ex-

perience shows that the time required for digestion varies with different individuals, so that no rule can be laid down.

The only rule to be given, as to quantity, is to stop short of satiety, either in drinking or eating: this point is easily ascertained; and by attention to it, as well as to mastication of the food, the digestive organs are never over-burdened, and not so liable to disease.

This brings us, gentlemen, to the application of these principles to the treatment of disease, or the diet of the sick.

There is a principle, which you have often heard mentioned, the *vis medicatrix naturæ*, which has a great bearing upon the subject under consideration. You will *generally* find that Nature will dictate the kind of diet necessary: I say *generally*, because the exceptions are only sufficiently numerous to establish the law. In most diseases, for example, there is a persistent and characteristic loss of appetite; and, while there are exceptions, you will generally find that the less aliment your patient takes during the acute stage of the disease, the better is it for the course and successful termination.

Bearing, then, this principle in mind, you will, I trust, be ready to be an assistant of Nature, rather than inconsiderately to interfere with her dictates. Be satisfied to follow her leadings, unless you find that they have been so perverted by disease as to be unreliable; and, in order that you may be able to judge of this, I shall lay down some general principles to guide you.

The subject may very properly be divided into three classes:

1st, *Antiphlogistic*; 2d, *Tonic*, including the diet of convalescence; 3d, *Alterative*; and under each division such facts will be mentioned as tend to modify the general rule.

1st. *Antiphlogistic*.—This term signifies literally “opposed to inflammability,” and is probably more appropriate in reference to diet, than to other remedial means. In order to enter understandingly upon the subject, let us glance briefly at the pathology of inflammation.

The characteristic symptoms of inflammation are heat, red-

ness, pain and swelling, all of which, however, are caused by the same condition, viz. increased flow of blood to the part inflamed. But upon an examination of the blood itself, we find a very marked and decided increase in the amount of *fibrine*. Now, this fibrine, you remember, is the organizable constituent of the blood, and goes, to a great extent, to the repair of the muscular tissue of the body. In choosing the diet, therefore, for a person laboring under any inflammation, we must have reference to these two objects, viz., to diminish the amount of blood circulating through the inflamed part, and to decrease the proportion of fibrine of the blood.

This can be done to a great extent, and other remedial efforts materially aided by cutting off the supply of nutritive matter, just as the gas light may be diminished by decreasing the amount of gas flowing through the tube. Were your patient allowed access to an indiscriminate and unrestrained diet, composed of nitrogenous and oleaginous matters, you would speedily find the amount of fibrine increased, heat, &c. becoming greater, and all the symptoms materially aggravated.

To illustrate this position. The success of the treatment of coryza (ordinary inflammation of the mucous membrane of the nose), by means of a total abstinence from both food and drink, depends entirely upon this fact; for the supply of nutritive matter being withdrawn, the amount of blood circulating in the system is necessarily decreased, and with it the actual amount of fibrine. What, therefore, will cure a simple coryza, will be found a valuable adjuvant to the treatment of inflammation of more important organs.

The diet of inflammation, therefore, must be next to starvation. The less the patient eats the better, and I believe this is true also of drinking. I would not deprive him of the means of allaying his thirst and relieving the burning of which he will complain; but as the relief is but temporary, and as effectually accomplished by a piece of ice or a mouthful of water as by a gallon, you can obtain both ends by allowing him to swallow these from time to time. If, however, it should be deemed necessary to give some slight nourishment, barley

water is as fair a compromise as can be made, for by the addition of ice, it acts both as food and drink.

It is very much the custom to give soups, slops, &c., the chief of which, for particularity, is chicken water, which latter has been appropriately described as hot water through which a chicken has flown. Discard the whole of them from your diet scales; make no such compromises with domestic superstitions, for they can do no good, and must do harm by loading the stomach with water, at a temperature neither grateful nor healthy. If your patient needs nourishment, give it in a form that will accomplish the object; and if he does not, withhold it until he does, in spite of all that domestic practitioners of the female sex may say.

The attentive physician will readily see when this course has been carried sufficiently far. Should convalescence approach, the diet will be modified as hereafter to be described. Should the powers of the system flag, it will become necessary to give some material to support the failing strength of the patient. For this purpose, beef tea is admirably adapted, containing, as it does, the nutritious part of the beef in a form easily digested and very concentrated. It may be repeated from time to time, according to circumstances, and should it be deemed expedient, may be given in connexion with wine, brandy, &c. Remember the danger you are incurring; but you must choose between the evils of death from the disease or death from exhaustion, the former being possible, the latter almost inevitable. If beef is not convenient, any meat prepared in the same way will answer.

What I have just said of the secondary treatment necessary, is true also in many cases, from their incipency; when it becomes necessary to blow hot and cold, as in the case of inflammations in inebriates, where you have the disease to fear on one hand and the habits on the other.

If the proposition with which we started is true, especially is it so with inflammation of any of the digestive organs. Rest, absolute and entire, as far as possible, is always necessary for any diseased organ; and as the material for nutrition

is brought into the system by the stomach, bowels and their assistants, it follows that anything, however nutritive, introduced into the stomach during inflammation of these organs, must have a deleterious effect. In gastritis, for example, the first effect of inflammation is to diminish the secretion; and food then introduced is either not digested, or if so, calls the organ into exercise and increases the excitement already existing, by breaking in upon its ordinary rest; if not digested, it acts as a source of irritation, and if not speedily removed by an effort of nature, must add to the inflammation. This condition always complicates the treatment of these diseases. Should it, however, become necessary to administer nourishment, the various mucilaginous drinks, as tapioca, sago, arrow-root, &c., may be given, the bad effects of which are counteracted by the soothing effect upon the inflamed mucous membrane. I believe, generally, it is better to prepare them with water than with milk, though this must be determined by the amount of support required; they may be sweetened and flavored to the taste, and if necessary, wine may be added.

The diet necessary in chronic inflammations is essentially the same as that required in acute, bearing in mind the length of time necessary for a cure, and impressing upon your patient the necessity for a persistent diet. Let it be strictly nutritious, discarding all matters not easily digested and not nutritious, such as pastry, desserts, &c. An error of diet once, may overthrow the labors of months. This brings us to the

2d. *Tonic Diet*.—This may be divided into that necessary for acute and chronic diseases.

The first is the diet for a patient convalescing from an acute attack of any inflammatory disease.

As soon as inflammatory action has subsided, the powers of the system are rapidly depressed, upon the principle so often enunciated to you, that any excitement is followed by a corresponding degree of depression; and to this cause must be added the debilitating effects of the remedies employed. It is necessary, therefore, to resort directly to means which will support the system until the powers of nature are sufficiently

recovered to carry on the functions of the body. But, bearing in mind that the digestive organs sympathize with the general debility, you will not give any solid food as yet, for fear that, not being digested, it may act as a source of irritation. It will be necessary, therefore, to begin with nutritious articles, which are, at the same time, easily digested; such as beef tea, *strong* soups, boiled rice, or farinaceous articles. I have generally found that a slice of *cold* light bread or toast, with a soft-boiled egg, was much relished when convalescence was fully established, and easily digested. To this, after a few days, a little butter might be added, and thus gradually and step by step you will bring the patient back to his ordinary diet. If there is any thing not likely to be injurious to him, which he craves, you may permit him to have it, carefully watching the effects of each change of diet.

You will be frequently asked if your patient may have fruit, vegetables, &c. A fully ripe peach may be eaten with impunity, and an apple roasted and deprived of the peel and core, may be innocuous; but, of the vegetable class, there are few, if any, which are admissible, either in the acute attack or convalescence, and I would rather err on the side of prudence where there was any doubt.

Typhoid fever and its complications nearly always demand a tonic diet from the commencement. Here you have a debilitated condition of the system connected with a lesion of the alimentary canal, which combination requires great care and judgment in the choice of articles of food. The blood shows a diminution in the relative amount of fibrine; hence you must, if possible, give those articles which will overcome this loss; and, on the other hand, you are forbidden to use solid food, lest you may force the ulceration of the bowel to perforation. In this condition I have generally used some one of the animal teas, or a thick soup, or some of the farinaceous articles of food, though I think the animal matters are preferable to these latter. Sometimes you will find that a little panada may be acceptable to the patient, though I would advise that all of these articles may be made thick, so that the

relative amount of nutritive matter to the water may be as great as possible.

I can, of course, not give you any positive directions as to the condition of system requiring alcoholic stimulants. These, like the food, may be given moderately and gradually, the effects carefully watched, and by those effects you must judge of the necessity existing for them. There is, however, some difference in the effect of different varieties of stimulants. Wine is not so powerful as either whiskey or brandy, but is more apt to disagree with the stomach by becoming sour. Should this occur, of course it must be discontinued; brandy and whiskey both seem to have a more positive tendency to the arterial system than wine; which last has a more general, diffusible effect. Porter, ale, and all the malt liquors are better suited to chronic cases than to acute, since by their very constitution they are heavy and gaseous, and hard to digest; their action also is more tonic than stimulant.

What was said of convalescence from inflammatory attacks is doubly true here, the great debility of the system requiring a careful transition from the sick-room diet to that of health.

I have taken typhoid fever as a type of acute disease, requiring tonic diet, and I shall now take phthisis pulmonalis as a type, both of the chronic asthenic condition, and because of its very great importance.

A half century ago galloping consumptions were very common, and six crackers a day and a cup of black tea were the only sources of nourishment given to counteract the wasting tendency of this enemy of mankind. Now, however, "a change has come o'er the spirit of our dream," and beefsteak and porter have taken the place of the scant rations of the old system. The increased knowledge of the pathological conditions of consumption would make us disgracefully culpable were we to keep a phthisical patient on the diet formerly prescribed.

Now phthisis is one of those diseases belonging to the class of cachexial, or perverted nutrition; the balance between the functions of waste and re-production is essentially lost, and

continued and increasing emaciation is the result. Whatever may be the cause of this condition, one thing is certainly true, that no remedy has ever been found which will remove it, though innumerable ones have been tried; cases have certainly recovered of themselves, but none were ever *cured*. The only chance for the patient is, not in the *materia medica*, but in a generous diet.

Experience has shown that the only remedy is found in free living; in addition to active exercise, more especially on horse-back, we must see that the patient has the best of food and a plenty of it. And while it is unnecessary to put any limit either on quantity or quality of food, he ought as far as possible to shun all trashy sweets, &c., and eat good, wholesome meat, bread and vegetables. Here you will find the general rule of the dictates of nature will fail, and it is necessary to overcome that depraved condition of the system which takes away the appetite for all food, even when giving way under a fatal disease.

Upon the great necessity for nourishment I believe that most, if not all the remedial effects of cod liver oil depend. A patient whom you cannot persuade to sit down and eat a hearty meal, will readily take a spoonful of oil to cure himself; it possesses great nourishment in small compass, and is, therefore, easily retained. The system, after a little while, its tone being restored by frequent doses of the highly nutritive substance, responds to the aid thus granted in an increase of appetite and digestive powers. It also acts by furnishing for the lungs, what must otherwise come from the tissues. As a proof of this, experiments have been tried with other fats, with great success.

The theory of Dr. Hooker, alluded to before, demands some notice. The difficulty of establishing the data upon which he bases it is, in our opinion, insuperable. And I confess, that so far as my experience goes, it is not carried out. I have no doubt that meat is absolutely necessary, but that it is necessary to be fat meat instead of any other oleaginous matter, is a question of great doubt. Few persons would try it, and few

stomachs could bear any considerable quantity of fat meat upon it without a due admixture of the lean, or some nitrogenous compound.

As an adjuvant to the mere food, we have alcoholic stimulants, any of which are good, but probably porter, ale and the other malt liquors are the best. I would not limit the patient to the use of these only once a day, but as often as may be necessary ; indeed, I have known cases kept alive for years by the use of liquor in quantities that in a healthy person would be inordinate and intoxicating. It acts as a general diffusible stimulant, increasing the appetite and, temporarily, the strength. A strong milk toddy, given at bed-time, has the effect of giving a comfortable night's rest, diminishing the cough during sleep, and assisting the lungs to throw off their accumulated secretion in the morning.

In a word, let your diet in phthisis be directed to overcoming the vice of system, by increasing the red corpuscles and fibrine of blood. The same general rules, applicable to consumption, must be carried out in the other cachexiæ, as for example, scrofula and cancer ; in both of which, experience has shown that a restricted diet will not answer, and that the only hope of cure is in a liberal allowance both of quantity and quality. We pass now to the third class.

3. *Alterative Diet.* The first variety of diet to be mentioned under this head, is that for dyspepsia, a disease, which probably is of as difficult management as any other, and the diet for which is as hard to determine. It might, possibly, have been mentioned before, but the three kinds of functional, inflammatory and cancerous, rather place it among those diseases to which particular rather than general rules apply. And it is especially injurious to follow nature in her dictates, since she frequently counsels to improper indulgence, both of quantity and quality.

There is no essential difference in the diet necessary for the different varieties, and on the other hand there is not any diet which will be certainly suitable to all three. You can, at

last, regulate the diet of dyspeptics only by attention to the general rules, before laid down for a healthy person.

There are a variety of breads, which have been offered to dyspeptics as certain remedies, from that made mechanically, to that which acts mechanically. Bran bread, rye bread, &c., have been supposed to exert a good influence in dyspepsia, by keeping the bowels open, by their mechanical effect of irritation, an object very desirable, if it can be attained, but I doubt very much the propriety of giving indigestible food to a stomach already weakened, and whose complaint is, it cannot digest. Does it not seem plausible, that the constipation is due rather to the disease of the stomach, than to any lesion of the bowels, which could be overcome by mechanical irritation? I believe cold wheat bread is the most digestible, and therefore the best: I advise it to be cold and stale to some degree; because, by the fermentation set up by yeast in making bread, a number of gases are disengaged, which will distend the stomach to a great degree, if taken hot; but when the bread is cold these have disappeared. The bread should be sweet, since any acid matter must derange the stomach. The bread made by mechanical means is used by many physicians, they attributing to it no property beyond that of sweetness.

The drink should be milk, or, what is better if it can be had, cream. Tea and coffee should be entirely banished from his diet; and if the milk or cream disagree, then let him use water.

No meat should be used, for the reason that the weak stomach would find difficulty in digesting it, and because the constituents of it can be introduced better in other forms.

The food should be taken little at a time. Dr. Watson of London mentions the case of a young girl, who, suffering from dyspepsia, was not conscious of having retained anything on her stomach for twelve months. Knowing, of course, that this was impossible, he rightly supposed that though the stomach rejected most of the milk drank, yet that some was necessarily retained, he adopted the suggestion of nature, and gave nourishment often and in small quantities, with perfect

success. The explanation is rational: the stomach would retain and digest what it could, and reject the rest, and by only giving as much as it could digest at a time, a cure resulted.

Alcoholic stimulants must be entirely discarded, except in some few—very few—cases, caused by debility of the system; then they may be useful, the distilled liquors being preferable to the fermented.

Fruits and vegetables should not be used, because they are so apt, even in health, to give rise to flatulency.

There are cases, however, in which this plan will not succeed; and you must then be governed by circumstances—try various kinds of food, one at a time and in small quantities, until you find what does agree with your patient. In these extreme cases, in which death is at least probable, you may follow the dictates of nature with good result. Dr. Chamberlayne late Professor of Materia Medica in the Medical College of Virginia, was in the habit of telling his class of two cases of dyspepsia, one cured by *fried oysters* and the other by drinking vinegar; both remedies being suggested by nature, and acceded to, only as the fancies of dying men.

It is, however, especially in diseases of the urinary organs, that we see the good effects of diet: were this more attended to in health, we would see less disease of the kidneys and bladder; and were it more attended to when the disease is initiated, we would have fewer cases of injury from so called *antilithics*. Nor is this unreasonable; recalling for an instant, the relation between the stomach and kidneys, the one the receiver, the other the eliminator, you will readily comprehend that improper food, if not carried off by the bowels, must be eliminated either by the skin or kidneys, and most generally by the latter, giving rise to one or more of the renal diatheses.

The deposition of lithic acid takes place, whenever food too rich in nitrogen is taken into the system, or rather whenever the nitrogen of the food cannot, for any cause, be carried off by the other excreting organs. The class of persons, among

whom it is found, will plainly show its cause, and point to the remedy. It is supposed to be the acid of gout, and is one of the numerous evils attending what is called "*high living*." Rich meats, brandy and wines, especially Madeira, are among its exciting causes.

The remedy is simply adhering to a simple, but nutritious diet, choosing, as in dyspepsia, those things most likely to agree with the stomach, *e. g.*, the white meat of fowls, roasted mutton and vegetables, of which rice is the most digestible, stale bread and cream, or perhaps black tea. This course, combined with medical remedies to restore the condition already deranged by the vice of system, will alleviate, if not cure; but where the gouty diathesis is strongly developed, one error of diet will bring on the deposit and cause an attack.

Of the *oxalic* diathesis, too little is known to assign its cause, though it may be very materially alleviated by attention to diet.

The *phosphatic* deposit is generally found in broken down constitutions, and persons suffering from debility. Being the result of a depraved condition of system, it can be successfully treated by removing the cause, and substituting good wholesome diet, for a bad and insufficient one; only let it be proportioned to the condition of the stomach and its ability to digest.

I have thus, gentlemen, grouped in one view the different kinds of diet necessary for different conditions. I have been able merely to glance at the subject, for time has compelled me to compress into one lecture, what might be easily and profitably extended over several. I commend the subject to you, as one whose importance is only equalled by its interest, and the careful study of which will yield a rich reward in the pleasure it will give, and the facility which it will afford, in the practice of your profession.

ART. III. *A New Cause of Vesico-Vaginal Fistula*. By D. WARREN BRICKELL, M. D., of New Orleans, Professor of Obstetrics, New Orleans School of Medicine. *Audi alteram partem!*

IN the January, 1860, number of this journal,* there appeared a report of an operation for vesico-vaginal fistula, by Washington L. Atlee, of Philadelphia. The mode of operating and the manner of describing the process, are highly creditable to Dr. A., but in his attempted explanation of the cause of the fistula lie grave errors, the prompt exposure of which the status of our profession imperatively demands.

We have no personal acquaintance with any of the parties concerned; the reporter of the case is only most favorably known to us; therefore our criticism cannot savor of personality. We think a dangerous path has been opened for the unsuspecting footsteps of youth, and we would raise a warning hand. We think the highest interests of the most important branch of medicine are jeopardized by teachings like this, and we shall try to lay the errors bare.

Notwithstanding the patient's attending physician, Dr. Weatherby, plainly says that she was in the second stage of labor from 4 A. M. to 2 P. M. (and how much longer we are not told), and that during at least four or five hours of this time "the head seemed to be fixed permanently," was "jammed and immovable;" and notwithstanding Dr. W. says he gave ergot at 9 A. M., and that under its influence the uterine con-

* The article here criticised appeared in the late January No. of the American Journal of the Medical Sciences. Under the impression that this paper should appear before the same readers, it was offered to the American Journal, but was rejected, because it has not been their practice to publish reviews of original articles appearing in their pages. And moreover, because, although "the criticisms are probably just," "their tone is such as to forbid their insertion." The paper was then offered to the North American Medico-Chirurgical Review, but proved unacceptable, because they "make it a rule to publish nothing of a controversial character in the pages of our journal, especially when the criticism is made upon an article which did not originally appear in it." Comment is unnecessary.

actions became so urgent that he had to give opium to relieve the suffering of the patient ; yet Dr. Atlee leaves all this reasonable ground for accounting for the production of the fistula, and says it must have occurred before labor began. How?

Dr. W. tells us, at the close of his account of the case, that there was great anterior obliquity of the uterus, throwing that organ at right angle with the body of the woman ; and on this hook Dr. A. hangs the frailest theory ever promulgated.

He says: "The natural tendency of this position of the gravid uterus would be to elongate the vagina, and with it to drag up the bladder and curve it over the crest of the pubes. At the same time, the vertex of the child's head would necessarily rest upon the crest of the pubes, and be likewise firmly pressed against it by the posterior wall of the vagina, and the corresponding portion of the cervix uteri being stretched tightly over the face of the child. The anterior wall of the vagina and the coats of the bladder would consequently be compressed between two hard bodies, and this compression being constant, and its force constantly increasing as pregnancy advanced, it may be readily understood how the vitality of the vesico-vaginal wall was destroyed, even before parturition commenced"!!

What says the reader? Will he not agree with us, that the head of the child of the woman still in process of gestation is *in utero*? and being in utero, that it is therefore above any portion of the vagina? and being above the vagina, and dragged over so as to rest on the crest of the pubes, that it cannot possibly infringe on any portion of the *vesico-vaginal* wall—in which wall only can *vesico-vaginal fistula* occur? If the imagination cannot carry you at once into the reality of the proposition, go to the cadaver, take hold of the uterus, pull it out of the pelvis, watch the location of the *vesico-vaginal* wall, and you will see what we mean. Not only the anatomy of the parts involved contradict this part of Dr. A.'s theory, but the plainest physical laws show that the head of the child can infringe on the vesico-vaginal wall only when *descending* through the pelvis, and that to draw the parts

upwards while the head remains in utero can never result in like infringing. Under the anterior obliquity theory of Dr. A. there is but one portion of the bladder that could be caught under the head, and that is the portion of the organ in the region of its summit, and where it is covered with peritoneum, and a fistula occurring here would necessarily communicate with the cavity of the abdomen.

But what says the reader? Does he really believe that the head of a child could press on the crest of the pubes, as described by Dr. Atlee, and produce sloughing of the tissues? Will he, like Dr. A., forget that there is first liquor amnii in the uterus to largely counteract the specific gravity of the child? and then forget that there is the thick and elastic uterine tissue between the vesical wall and the head, with an anterior vesical wall between the posterior wall and the pelvic bones? Will he believe that a head that is absolutely *in utero* can be pressed against the crest of the pubes by a posterior vaginal wall that is positively below it and not touching it? Or, again, will he believe that a child that is lying at a right angle with the woman's body can have the *vertex* of the head pressing on the crest of the pubes? What a mess of anatomical and physical improbabilities!

But Dr. A. attempts to corroborate his theory by some urinary hypotheses. Let us examine them and see if they will hold water.

First, he says that Dr. W. tells us that two or three days before labor set in, there was incontinence of urine, and on this he bases a probability of fistula existing before labor. Is this, then, the first time that a woman on the eve of parturition has been known to be the subject of incontinence of urine? Surely not, according to our humble experience. Or, if this incontinence be a sign of fistula before labor, then is fistula before labor by no means uncommon. But Dr. W. does not say that the patient was leaking constantly before labor, or even during his attendance on her in labor. If she was, he should have known it. Neither are we told of any suffering during gestation, that must have been the accompaniment of the process

which is supposed to have produced such loss of tissue. Surely Dr. A. cannot pretend to urge that such a result could ensue without producing local and general disturbance sufficient to have elicited attention. Nor are we told of any of the harassing symptoms connected with the constant dribbling of urine over the external tissues of the woman, especially during warm weather. It has been our lot to pay considerable attention to these cases, and we have yet to see a white woman who does not suffer in this respect.

Second. Dr. A. lays great stress on the fact that for two or three hours before Dr. Clark arrived, the patient passed no water, (we are not told when or how often before that she did pass it,) and when they passed the catheter there was no water. Is this the first time that scanty secretion of urine has been noticed in protracted labor? Surely not, according to thirteen years' personal experience of our own. And more, may not the attending physician have erred in supposing that he really penetrated the bladder? The facts subsequent to delivery imbue us most thoroughly with this idea.

Third. On the very ground on which we should have predicated the non-existence of fistula—viz., the natural evacuation of the bladder during a number of days subsequent to delivery—Dr. Atlee plants himself, and boldly advocates the pre-existence of the disease! What does he say? Why, that the tumefaction of the vesico-vaginal wall, consequent on the protracted labor, *closed* the fistula during these days subsequent to delivery, and on the same principle that the water flows through the urethra of a man during the condition of tumefaction of the wound made in the bladder for the extraction of stone! Does any unbiassed mind receive this as a truism? Is the analogy to be admitted for one moment? Does Dr. Atlee himself really believe that a fistula in the trigone region of the bladder, "*at least one inch and a quarter in diameter in all directions,*" (we italicise his words,) could be closed even for a day on this principle? Indeed, is not the similarity of the two cases practically unworthy of a moment's consideration? In the one case there is only a severing of tis-

sues, the severed edges coming in contact the moment the knife blade is withdrawn; in the other case there is extensive loss of tissues, leaving the edges of the fistula "one inch and a quarter" distant from each other in all directions. Whose imagination can stretch so far as to grasp at this last proposition of Dr. Atlee? Surely not that of any man who values facts more than fables.

Dr. Atlee closes by saying, that if his explanation be "the true and legitimate one" of the cause of the fistula in question, then "it ought to be a perfect vindication of the skill and carefulness of the experienced and excellent accoucheurs who managed the case." We quite agree with him; but that "if" is utterly insurmountable. The accoucheur no doubt did his best; but that fistula occurred from precisely the same cause that has produced all others in labor, and until an entire revolution in the mode of managing difficult labor takes place, we shall have hundreds annually added to the black list of so-called accidents which disgraces the records of Obstetrics. Amid the shortcomings of obstetric practitioners, let no man rummage for excuses, but, with the great purpose of material improvement ever before us, let us reduce our practice to the strict line of conservatism—let us flee from the dogma that woman in labor can accomplish wonders without assistance, and to these women, and to the children within their persons, we shall do far more good than has heretofore been done.

If Dr. Atlee will look back into the pages of the New Orleans Medical News and Hospital Gazette, he will find a clear account of a case of vesico-vaginal fistula caused by other means than the pressure of the head in labor. In the meantime, we must insist that his explanation is neither "the true" nor "legitimate one." He has performed a handsome cure, but has made out an ugly case.

ART. IV.—*Case of Burn, Successfully Treated by Alumen Pulv.* Reported by WM. MASON TURNER, M. D., Petersburg, Va.

THINKING the report of the following very severe case of burn, which came under my professional observation, especially insomuch as relates to the final, simple, successful, therapeutic means employed might be interesting to some of the readers of the Journal, I am induced to give it, and I do so, as briefly as possible.

On the 21st of February of the present year, I was called to see J. M., who was suffering, as the lad told me, from a "*monstrous, terrible burn.*" On reaching the bed-side of the patient, I found, indeed, a most terrible burn. The history of the case revealed the following: Just one week previous to the date at which I was called, M., (who, let me remark, was, on the morning of the day he was burned, discharged by me as well, having been stabbed severely in the right breast,) in a state of beastly intoxication, endeavored to walk across the room in a drinking saloon. In the attempt, he fell over a stove, and upset on himself about two gallons of boiling coffee. The steaming liquid saturated his pantaloons, a heavy, stout woolen pair, and coursed over him from the sacrum, over the right gluteal region, reaching over on the abdomen to the region of Poupart's ligament, down to the external abdominal ring. The exterior portion of the thigh and hip was burned for the length of fifteen inches, with an average breadth of eight and a-half inches. The left thigh was burned, commencing under the scrotum, and including its entire interior surface, reaching in greatest length to the external portion of the patella, and ranging in breadth from four to nine inches. The anus was partially involved, and I feared much a cicatrix and consequent closure of that organ. A more foul and offensive wound I never before saw in the wards of any hospital. The odor was almost unbearable. The man was quivering with pain, wore an anxious look, and had a small, thready, irritable pulse.

He had not slept for eight nights, or since the night of the accident. He had, of course, happily survived the shock, which, owing to his inebriated condition, was not serious to the general system. So drunk, in fact, was the man, that the mad excitement induced by liquor buoyed his spirits above the pain resulting from the severe wound, and under this influence he walked alone to his house, a distance of half a mile or more, with the saturated, steaming pantaloons still about him. His repentant feelings and shame, at having so soon to call me again, (his stab was received in a drunken brawl,) prevented him from sending for me, until having tried several old women's different absurd remedies, and finding himself getting no better very rapidly, his fears overcame his scruples, and I was called just one week after the occurrence. On taking down the coverlet to examine the wound, a perfect avalanche, if I may so term it, of *odeurs mauvaises* rushed over me, and, although accustomed as I am in my profession to every describable stench and smell, I came near succumbing to this attack. The entire surface of both wounds was covered over with a thick, hard crust, which, upon inquiry, I found was composed of flour, resin and soot. The edges of the raw wound, alone visible, presented very foul, pale, sickly granulations in great exuberance, while the entire periphery of the burn presented a purplish, erysipelatous appearance, which did not improve the ground for a favorable prognosis in the premises. Every thing was against the poor man; he was in the most abject circumstances; his conveniences were none; no food more substantial than corn meal; his nervous system shattered, and his vital forces on a fast ebb. Under the circumstances, I ordered the following:

R—Tinct. cincho. \mathfrak{z} vi;

Sulph. acid comp. \mathfrak{z} i.

S.—Tea-spoonful four times a-day.

Table-spoonful of whiskey, if any signs of immediate prostration.

R—Sulph. morph. grs. ss.;

Ros. conserv. q. s.

Ft. in pil. No. 1. S.—Take at seven o'clock, P. M.

And the following local applications : Light-bread poultices to the hard crusts, to favor their removal, and straps of linen, cut to fit the naked, granulous periphery of the wound, wrung out in carron oil, to be removed three times a-day—the last application to be followed by linen strips, spread very lightly with Kentish ointment. The constitutional tonic treatment and the exhibition of the opiate every night was continued—the latter being varied with morphia, hyoscyamus, lactucarium, &c. The poultices and different linen strips were also continued. At the end of the week, the poultices, aided by a slight dissection with the knife, had the good effect to bring away the ugly crusts, and then the entire, repulsive, flabby, granulating, offensive surface was presented. The edges of the wound in the meantime had shown no signs of taking on a healthier action, and the erysipelatous purple was spreading. The wound was suppurating profusely, and this, connected with the debilitated state of the system, I feared would occasion pyæmia, in spite of my tonic treatment. As the ugly sloughs came away, at least a handful of decayed muscular structure, in a *cooked* condition, was wiped off with a sponge. The process of dressing occasioned much and intense pain to the sufferer, and I had freely to administer liquor, the only quick diffusible stimulant at hand, very frequently. I applied linen rags, wrung out in carron oil, three times a-day, covering all over with light oil silk, to prevent any possible contact with the air—the last application to be followed by Kentish ointment. I continued this treatment for several days, during which time I was compelled to plug the anus with a piece of *charpie* saturated with ol. oliv.

At the end of five or six days, there were no signs of improvement. Thinking the case was rendered worse by frequency of dressing, I ordered the saturated rags to remain for a longer time; but pus was secreted so rapidly, and produced such an offensive smell, that *per force* it was necessary to change the strips. Finding no good result follow this local treatment, I tried the solution of the per manganate of potash, which I know to be much esteemed, especially by English sur-

geons; but with this effect only, it rendered the wound perfectly sweet—a happy riddance truly! The wound showed no signs of increased healthy action, but remained perfectly *in statu quo*, while the man was sinking every hour. I endeavored to touch the edges of the wound with the solid stick of nitrate of silver, but on the slightest application the patient swooned, and at the mere mention of *caustic* afterward, he showed not only signs of fainting, but symptoms very similar to convulsions. Of course this, the best topical application, was useless. I next tried to improve the patient's general system still more by adding a preparation of iron to the tonic. I was already administering, and treated locally by dusting the part with powdered calamine, covering the wound over with rags, spread with simple cerate. I flattered myself that under this treatment the wounds did improve; but the symptoms were delusive, for the granulations disappeared in a measure, but a day or so afterwards, while the patient attempted to move himself in bed, a severe hemorrhage took place from both burns, and reduced his already debilitated system. Yet enjoining on him perfect rest, I continued the calamine dust, and simple cerate treatment. After a fair trial, however, I came to the conclusion that it was unavailing. I tried Goulard's cerate, and strapped the wounds with adhesive plaster; but without any satisfactory result. In the meantime, the patient told me that two or three hemorrhages had taken place from the inner face of the thigh, and that he was much exhausted by it. I did not examine the spot particularly from which he said the bleeding seemed to proceed. One day I was dressing the wound, and requested him to move himself, if possible, and get in a more favorable position. He made quite a violent exertion, and as he did, quite a jet of dark blood burst from one single point on the inner face of the left thigh. I had no styptic at hand. I tried pressure and cold water, but without success. On inquiring, they told me they had some alum. This I had powdered finely, and applied to the bleeding place, and had the satisfaction in a few moments to staunch the hemorrhage. On examination of the bleeding place, I found a soft, fluctuating tubercle, bluish in

appearance, about the size of the end of the little finger. It protruded from under the internal edge of the quadriceps femoris muscle. The wound was of such a depth that I actually at one time feared that the hemorrhage came from the internal saphenous vein, and that the bluish tubercle was the vein itself. However, after having staunched the hemorrhage, I applied a small compress, and bound it *in situ* by an adhesive strap. Before closing the dressing, merely to see what effect it would have, I dusted the edges, for the depth of an inch, throughout the entire periphery of the wound, with powdered alum, covered it with the usual dressing, and left. What was my joyful surprise next day to see the portion over which the alum had been sprinkled, wearing an altogether different appearance—the foul granulations cut away and the edges of the wound evidently disposed to go up. I could well exclaim *eureka!* Fearing from the *commonness* of the remedy, that the man would not be impressed favorably with this *trifling* treatment, as he would and *did* term it, I determined to bring powdered alum with me myself the next day, taking care to have it duly labelled, &c. All practitioners know the power of mind over matter, and that its nowhere exhibited so strikingly as in the practice of medicine. I contented myself with dusting the edges somewhat again with alum, and applied the usual dressings. I continued with the alum powder, dusting the entire surface, and instead of covering with rags spread with simple cerate, I applied the cerate of the impure carbonate of zinc, or calamine cerate. This I did not spread on a cloth, but melted in a large iron spoon, and after the alum was well applied, I poured the melted cerate over the entire surface, and covered the wound with very light oil silk. From that moment I had no more trouble; the man slept so well that anodynes were dispensed with, and nature afforded rest, unaided. The tonic treatment soon produced an appetite, and rich food gave the patient good blood. The wounds healed very rapidly after commencing the alum treatment, and what is more singular, more fortunate, and very inexplicable, there was no *drawing cicatrix left*. The man is to-day in better health than he ever was, and can walk

anywhere he pleases. I ascribe the case wholly to the *accidental employment of alum*; and because it is a domestic remedy and easily procured, I have reported it, thinking some of the profession might be inconveniently placed in regard to medicine, as I was, and might be induced to give it a trial. From this one case of successful usage, I think it well worth one at least.

ART. V.—*Remarks on Amputation of the Foot, with Cases Illustrative of an Operation not generally performed.* By ALAN P. SMITH, Student of Medicine.

At the present time, all amputations of the foot are performed through some plane of the articulations of that member, as for example, those of Messrs. Hey, Chopart, Lisfranc, &c. These operations seem to me to have been introduced and to have found favor, principally on account of the difficulty attending their execution and their complicated arrangement. The one which I would suggest as far preferable on account of its many advantages, is, to turn back our flaps and make a clean and direct section of the bones. This, I think, has many advantages over the old method. In the operation of Mr. Hey, for example, we are obliged to follow with the knife the very irregular tarso-metatarsal articulations. Now, one great disadvantage that we here labor under is, that we thus lay bare nearly twice as much bony surface as by a direct section, because we expose, not only the anterior, but also the lateral articulating planes. Then, also, in the operation through the articulation, we have a cartilaginous surface instead of the granulating ends of the bones.

This is thought by some eminent surgeons so much to retard the closure of the stump, that they advise that the cartilage be removed by the knife or saw after the disarticulation has been accomplished. In proportion also, as we remove the ar-

tications, do we take from the elasticity of the foot. The advantages, then, of this section of the bones are as follows :

1st. It is performed with much more celerity and ease. 2d. Having less bony surface exposed, we avoid in a proportionate degree the danger of necrosis. 3d. The granulating ends of the bones more readily form union with the surrounding soft parts. 4th. The preservation of the articulations renders the result more satisfactory, and less limping and discomfort in walking. I would suggest this operation as not only the most eligible in amputation through the metatarsus, but think it far preferable, no matter at what part of the foot the operation may be required, after securing good flaps to make a section of the underlying bones.

The following cases are published as illustrative of the operation :

CASE I.—By Prof. N. R. SMITH. *Fritz* ; aged 40 ; sailor.

Eighteen months previously had lost all the toes of the left foot from frost-bite. After the separation of the toes, the heads of the metatarsal bones remained covered with an unhealthy blue cicatrix, which, being very tense, broke out on the slightest irritation into troublesome and painful ulcers, making it impossible to wear either shoe or stocking. It was considered best to remove the heads of the metatarsal bones. The patient having been brought under the influence of chloroform, flaps were obtained and the bones removed. The wound was dressed with linen strips, wrung out in cold water. The patient soon recovered, and was able to walk a little in two weeks from the date of the operation. He is now attending to his business, and walks with great ease and comfort.

CASE II.—By ALAN P. SMITH, student in the University of Maryland. *Mrs. Coleman* ; aged 50.

Had suffered for a long time from malignant ulcer of the toe. Two toes having been removed without accomplishing a

cure,—the disease making its appearance soon after the operation,—it was thought best to perform the preceding amputation, viz: by making a section of the metatarsal bones. The woman having been brought under the influence of chloroform, a very good flap was obtained from the plantar surface, and the bones divided about their middle. There was little hæmorrhage. The wound was then dressed with linen strips, wrung out in cold water. It has healed perfectly by first intention, the patient being able to use it somewhat, the second week after the operation.

SELECTIONS.

I. *Lectures on Experimental Pathology and Operative Physiology*, delivered at the College of France, during the Winter session 1859–60. By M. CLAUDE BERNARD, Member of the French Institute, Professor of General Physiology at the Faculty of Sciences.

ON THE RATIONAL PRINCIPLES OF THERAPEUTICS.

GENTLEMEN,—In the preceding lectures it has been our object to ascertain whether the exact counterpart of disease, in each of the numberless forms which it assumes, does not exist in the physiological state; but the discussion of this subject was only intended to introduce you to the study of another, and still more important question, which, from its very nature, is intimately connected with the foundations of Medicine itself—we allude to Therapeutics; a science, towards which converge all the other branches of the healing art; for the restoration of health to the sick must evidently be the ultimate end of all the physician's endeavors, and, in this manner alone can his noble mission be fulfilled.

From the earliest period to which our knowledge extends, we find the medical body widely differing in opinion on this point. Is it our duty to assail diseases with powerful remedies from the outset? Ought we not, rather, to watch the efforts of nature and avoid disturbing them in their course? The difficulty stands forth as prominently in our own time, as in

the days of the ancients; in other words, two opposite doctrines prevail among medical men on this subject; the expectant method is adopted by some, while active treatment is resorted to by others. The belief in Nature's healing powers is coeval with the birth of medicine; we find it expressed at large in the works of Hippocrates, and, to this day, the Expectant School lays it down as a fundamental principle. But the abstract meaning of all these terms require an accurate and distinct explanation; the word nature is frequently applied to that first cause, from which the entire universe derives its being; while, at other times, the word is used with reference to the universe itself,—to the visible effects produced, by the action of that everlasting force, upon inert matter; or, if we may be allowed to borrow the admirable expression of Spinoza, nature must be viewed both in an active and in a passive light; *Natura naturans* in the first case, and *Natura naturata* in the second.

The notion entertained of the *vis medicatrix*, or healing power, by the ancient physicians, was that of an innate tendency, or instinct, residing within the organs of the living body, and which, after they had been disordered through morbid influence, led them by degrees to resume their former state. It was therefore believed, that whenever man interfered in the struggle, his part should exclusively consist in favoring these efforts of nature which tend to the re-establishment of health; and thence arose the doctrine of crises and critical days, which, in the writings of Hippocrates, holds so prominent a place; he supposed that at certain stated periods, the evacuation of the *materies morbi* was spontaneously accomplished through some extraordinary discharge.

We therefore see that in this respect the doctrine of the ancients consisted in believing that when diverted from their proper course the biological powers would naturally revert to it without foreign assistance; and many of our cotemporaries still entertain the notion, making allowance, of course, for all the modifications which the progress of science has rendered indispensable. At various periods in the history of medicine we find these opinions in full vigor, although expressed under forms in accordance with the spirit of the age. Thus, the vital principle which pervades the living economy is by Stahl, attributed to the soul (*anima*), while Van Helmont calls it "the ruling power (*ἀρχή*); the idea remains substantially the same, and may be conveyed in these words: "Nature enjoys the power of restoring the health to the body without foreign assistance."

Clinical observation has evidently laid the foundations of

this doctrine; we daily witness the recovery of patients, who have not enjoyed the benefit of medical assistance. In this manner we see foreign bodies, when lodged within our tissues, expelled from thence by the natural process of inflammation, and the suppuration which ensues; and it appears perfectly rational to suppose that, in internal disorders, the noxious principle is likewise expelled through perspiration, or by means of some similar evacuation. When considered in this light, the symptoms of disease invariably exhibit a curative tendency, more or less remotely connected with the final end; every change that occurs in the patient's state, marks a new step in the contest; and, when death supervenes, it must be supposed that the powers of nature have been overcome by the intensity of morbid influence. Without adopting, *a priori*, similar views, several observers have endeavored to corroborate them by the result of direct experiments. M. Gaspard (of St. Etienne) having injected a decoction of putrid substances into an animal's veins, succeeded in superinducing febrile symptoms of the highest intensity, accompanied with shivering, prostration, and abundant alvine dejections; and, after a certain space of time, the animal recovered without undergoing any medical treatment. In other cases, the animal having perished, it was supposed, of course, that the *vis medicatrix* had been insufficient to conquer the foe. A large number of facts, resulting from clinical observation, might be satisfactorily explained by a similar process: the gradual resorption of pseudo-membranous exudations, and various other morbid productions, are instances to this purpose. It would, no doubt, be altogether superfluous to dwell any longer on this subject.

Far different, however, are the views of physicians belonging to the opposite party—those who adopt a more active course in the treatment of disease. Nature, say they, is frequently blind, and requires to be directed in her operations; in fractures, no doubt, she supplies the materials requisite for consolidating the broken shaft; but, if abandoned to the natural process, the limb is almost invariably shortened, and assumes a vicious direction, and the patient remains lame for life. Inflammation in the natural ducts generally brings on stricture; a result frequently attended with the greatest inconvenience, as in stricture of the urethra, and which, in certain cases, may ultimately prove fatal; when it occurs in the œsophagus, for instance, the obstacle created to the passage of food gradually causes the patient to die from starvation. The upper orifice of the larynx occupies a dangerous situation, in relation to the fauces; and the passage of an alimentary bolus into the windpipe is frequently the result of this unfortunate

anatomical disposition. It is, therefore, perfectly clear that nature may alternately be praised or blamed for what she does, according to the light in which her operations are viewed; and clinical observation supplies arguments to both parties.

It is evident that in various circumstances, and mostly in surgical cases, expectation becomes altogether absurd, and that an active mode of treatment is imperatively required. But, in a still larger number of cases, expectation is preferable; and the practitioner's skill is evinced by a judicious employment of the method adapted to each particular case. To establish, with absolute certainty, whether a given mode of treatment is profitable or injurious to the patient, is frequently impossible. Counter-proof would be indispensable to settle the question, and humanity prohibits any recourse to such a method of demonstration; besides, individual cases can seldom be fitly compared with each other, and cannot therefore, be safely brought together, for the purpose of establishing general rules. Dr. Gall, the phrenologist, wrote an interesting essay on this subject, in which he endeavors to determine, as far as possible, what is due to nature and what may be attributed to medical agency, in the cure of disease. Let us select two undeniable instances of these different conditions: in eruptive fevers the total inutility of our interference is evident; while, in cases of intermittent fever, we possess an agent of such indubitable energy, that to neglect to make use of it would be altogether unpardonable. We must, therefore, admit the existence of cases in which medicines do really appear to excite a considerable power over the progress of certain diseases; but to discuss the question is not our purpose for the moment; we shall direct your attention more especially to the manifestation of that physiological principle, whatever its intimate essence may be, which the ancients knew under the name of *vis medicatrix naturæ*.

It cannot be denied, that the physiological forces which regulate the vital functions, are endowed with a considerable power of resistance to the influence of external causes; and that whenever the natural state of things has been disturbed, they tend to re-establish the balance. Numerous instances of this might easily be adduced; we shall content ourselves with reminding you that, in certain of the lower animals, a lost limb is entirely reproduced; and that in the lowest ranks of the scale of being, the animal may be divided into several distinct parts, each of which becomes, after a short space of time, a complete individual of the same species. This singular phenomenon, which is known to occur in the hydra, and in various kinds of polypi, is equally observed, in a lesser

degree, in beings the organization of which is infinitely more complex: the lobster is often deprived of its claws, which are replaced by new ones; and, among vertebrated animals, the lizard frequently loses part of its tail, and a new one shoots forth in its stead. A similar tendency is found to exist among animals of a still higher type; thus, if in a dog the biliary duct is tied, when death does not occur from an effusion of bile into the peritoneum, a new duct is soon established between the two ends of the former one. We owe this information to Tiedemann and Gmelin; and I have more than once observed results entirely similar consecutively to the ligature of the pancreatic duct; shortly after this operation, the neighboring cellular tissue increases in density; the obliterated portion of the duct is resorbed, and a new channel is shortly established between the two separated extremities, exactly as in the previous case; the reproduction is even so perfect, that in making the autopsy, a few months later, not the slightest vestige of the operation can be discovered. Professor Sédillot in his experiments on the œsophagus, was led to the same conclusion. His original purpose was to prolong the duration of life in individuals affected with cancer, or obliteration of this natural passage from other causes. M. Blondlot's experiments on gastric fistula suggested the adoption of a similar method for medical purposes. The œsophagus having been tied in several dogs, they were fed, during a few weeks, through a perforation which had been established in the walls of the stomach; but after a certain lapse of time, the animals being unmuzzled, M. Sédillot was greatly astonished to see them actually feed by the mouth, and swallow large pieces of meat; on opening these dogs, it was discovered that the two separate extremities of the œsophagus had united again, and that a new channel was thus opened to the passage of food from the mouth. In the human species, considerable portions of tendons, or nerves, after being extirpated, are not unfrequently reproduced; in such cases, the newly-formed tissues go through the successive histological evolutions, which, in the embryo, are connected with the normal phenomena of evolution. During the period which I spent in the Hospitals, as a medical student, I had the opportunity of witnessing various cases of this nature; the following is one of the most remarkable: A large portion of the sciatic nerve had been removed in a young man who had submitted to the extirpation of a voluminous tumor in that region; on dissecting the tumor, the nerve was discovered within the morbid tissue; and as might have been expected, the corresponding muscles were entirely paralyzed, while sensibility was completely destroyed in those parts which

lie in connection with the lower portion of the sciatic nerve; yet, after a limited space of time, all these symptoms entirely disappeared, and the injured limb was enabled to perform its locomotive functions as before.

The solids of the human body enjoy, therefore, to a certain degree, that singular property of regenerating lost substance, which in the lower animals is so conspicuous; but it is in the liquids and epithelial exudations of the economy that this physiological force exhibits the full extent of its power. An animal may be daily bled to a considerable amount, without even appearing exhausted, if proper food be regularly supplied; and we know from daily experience how soon the loss of blood from hæmorrhage is repaired, provided the quantity of fluid thus suddenly withdrawn from the circulation is not so considerable as immediately to affect the vital powers. The blood, therefore, enjoys, in the highest degree, the property of repairing its losses; the same remark holds good of epithelial coating of mucous membranes; you are aware how rapidly excoriations are healed in the normal conditions of life. The perpetual renovation to which the blood and epithelial membranes are submitted in the physiological state fully explains the existence of a similar privilege in their favor.

You therefore perceive, gentlemen, that the mysterious powers attributed to Nature by the ancient physicians are readily explained by the normal properties enjoyed by living bodies, which even in a state of disease continue to exist, although hidden from view for a time; but as soon as an opportunity offers, they burst the veil, and make again their appearance. It will be readily understood why young patients in general enjoy greater powers of resistance to disease than persons of advanced age: the animal economy is endowed with greater energy at an early period of life, and struggles more vehemently to regain lost ground.

The critical phenomena described by old writers are in themselves the mere result of physiological agency. Have we not seen the yellow prussiate of potash, when introduced into the veins, pass into the urinary secretion, and thus escape from the economy? Is not alcohol, in the same manner expelled by the lungs? We must, therefore, consider the extraordinary discharge by which certain disorders appear to be cured, as the mere elimination of foreign substances, which, in health as well as in disease, are driven out of the body by a natural process.

These reflections, gentlemen, will no doubt convince you that the *vis medicatrix*, so long considered as a sort of mysterious power concealed within the living body, is the mere result of physiological properties, which, although cast into the shade,

as it were, by the disorders which attack the economy, still continue to exist, and, in most cases, gradually regain the mastery. When, therefore, death occurs, instead of saying that life has been overpowered, it would be correct to say that the physiological state has ceased to exist.

As to the part which the physician plays in the struggle, his duty consists in attentively watching the efforts of nature, and turning them to profit whenever a favorable opportunity occurs. Now, the agents through which an influence is exerted are medicines: to study the mode of their action, the power they exert, and the various cases in which they may prove either useful or injurious to the patient, will be the object of our next lecture.

II. *Discussion on Iodism before the French Academy.* By M. VELPEAU.

“I HAVE much used,” said M. Velpéau, “preparations of iodine, internally and externally. I deem it no exaggeration to affirm that, in twenty-five years, I have administered it to from 12,000 to 15,000 patients. This number will not be considered exorbitant when it is remembered that I exhibit this medicine in the hospital and in my private practice, in the space of a year, to more than 400 persons affected with hydrocele, serous cysts, bursæ mucosæ, sinuses, sanguineous or gangrenous ulcers, etc.; also in refractory tumors, scrofulous adenitis, cancerous affections, caries, tubercular disease of the bones, etc., etc.

“I must premise these remarks by declaring that I have never observed the formidable symptoms which Mr. Rilliet and other practitioners of Geneva have described under the denomination of *iodism*. I have noticed, in a great number of subjects, the phenomena pointed out by Lugol; irritation of the primæ viæ, coryza, obstruction of the nose, salivation; but, I repeat, never those general symptoms bordering on marasmus, never atrophy of certain glandular organs, which appear so common at Geneva. In two cases only, in a young man and woman, whom I treated by iodide of potassium, I noticed rapid emaciation, but without any other injury to health. Suspension of the remedy sufficed to restore regularity of nutrition and return of flesh.

"I cannot explain the extraordinary facts pointed out by Mr. Rilliet, but I do not therefore by any means deny them. I have too much confidence in our eminent fellow-practitioner's talents of observation, not to be convinced of the reality of his assertions and of the high degree of attention to which they are entitled.

"I am less well satisfied with his interpretations. I cannot admit that constitutional iodism can be connected either with the absence of iodine in the atmosphere of Geneva, or with the presence of goitre in the patients who came under Mr. Rilliet's observation. Might not more natural explanations be found in the doses of the medicine, in its possible adulteration, and especially in the enormous number of patients subjected to the treatment?

"I will not undertake to solve the question of the influence of doses. That iodine should occasion formidable accidents in infinitesimal doses, and should be, as it were, innocuous in enormous quantities, is a circumstance difficult to explain, in the present state of our knowledge; but it is not a fact that should surprise us, since every day the most usual agents of therapeutics afford us analogous instances. How is it that conjunctivitis is promptly cured by instilling, for four or five days, into the eye one drop of a solution of 1 gr. of nitrate of silver in 1 oz. of water? How is it that calomel, administered in very small doses, according to Law's method, and mercurial frictions on the skin, which allow but infinitely small proportions of mercury to penetrate into the circulation, produce prompt salivation? But a short time since I observed, within a few hours, in a young woman, most intense stomatitis with ptyalism, which lasted ten days, in consequence of a very superficial application of the acid nitrate of mercury to an ulcer of the leg as large as a florin-piece. In presence of these facts, which are familiar to all practitioners, may it not fairly be inquired whether it might not be the same with iodine as with hydrargyric preparations?

"Secondly, I opine that the number of patients subjected to the treatment must be taken into consideration, and this Mr. Rilliet has not done. He has observed twenty-three cases of constitutional iodism, but he does not state in what proportion of patients. This is a consideration of paramount importance; for it is obvious that the result will lose much of its value and all its strangeness, if it has occurred but very seldom in the hundreds, or perhaps thousands of cases under Mr. Rilliet's care. I regret, therefore, that, instead of confining himself to the statement of gross numbers, the physician of Geneva has not supplied us with statistics accurately recording

the proportion between the patients subjected to iodic medication, and those who became affected with iodism.

“Upon the whole, the medical practitioners of Paris have never observed constitutional iodism; those of Geneva have sometimes met with it; to what is this difference referable in the mode of action of the same therapeutic agent? The explanations hitherto brought forward appear to me inadequate. It is a fair subject for study and experiment, and one I deem worthy of the attention and solicitude of the Academy.”

III.—*The Laryngoscope.* (From the Berlin correspondence Medical Times & Gazette.)

As far as I am acquainted with the periodical literature of our profession, no notice has as yet appeared in your columns, or in those of your cotemporaries, with regard to the highly practical results obtained on the Continent by the use of the laryngoscope.

Having had occasion to convince myself of the comparative facility with which the larynx can be explored by means of this simple contrivance, I feel confident that its importance for the diagnosis of laryngeal disease cannot be overrated, and it will be a mere truism to state that we shall be able to attack affections of the larynx with far greater discrimination and success, if the uncertainties, inseparable from a symptomatic diagnosis, can thus be replaced by the precise results which a distinct view of the affected parts must afford.

A few weeks ago, I was present at a post-mortem of a phthisical individual, whose larynx had been carefully examined, a short time prior to decease, by Professor Traube. The changes found in the larynx bore testimony to the accuracy of the results obtained by laryngoscopic investigation. The following remarks on the instrument, and its application, are mainly extracted from a monograph, published in the early part of the year, by professor Czermak, who, together with Dr. Türk, of Vienna, has the great merit of having re-directed the attention of the profession to this important means of diagnosis. Indeed, these Viennese physicians may be said to have re-invented the larynx-speculum. Apart from its decided practical usefulness, the fact of the laryngoscope being originally an English invention ought to stimulate English surgeons to take an active part in the reform of laryngo-pathol-

ogy, to which the general application of the instrument is likely to lead.

In Liston's "Practical Surgery," page 417, we read, under the head of "Ulcerated Glottis," the following remarks: "A view of the parts may be sometimes obtained by means of a speculum,—such a glass as is used by dentists,—on a long stalk, previously dipped in hot water, introduced with its reflecting surface downwards, and carried well into the fauces." This pregnant hint of Liston's remained unnoticed till 1855, when Garcia published a most valuable series of auto-laryngoscopic investigations, instituted for the purpose of elucidating the mechanism of the human voice. In these experiments the image of the larynx was reflected from a mirror placed against the soft palate, so as to be received upon a second mirror placed in front of the observer (auto-laryngoscopy). An elementary knowledge of catoptrics will suffice to explain the principles upon which Liston-Garcia's method of investigation is founded. The examination itself is conducted in the following manner: A metallic mirror—varying in size from six to fourteen lines in diameter, in shape either square with rounded edges, as recommended by Czermak, or oval, according to Türck's proposal, or, as it has been found very convenient by Dr. Levin, of Berlin, semi-circular, with a concave inferior margin—soldered to a slightly flexible metallic handle, is to be introduced into the well-opened mouth, and fixed in such an angle against the uvula and soft palate as to throw incident luminous rays upon the larynx, and to reflect an image of the parts thus illuminated into the eye of the observer. To prevent the mirror from becoming dim by condensation of vapor upon its surface, it is necessary to warm it previous to introduction by dipping it into hot water or holding the unpolished surface over the flame of a small spirit-lamp. Garcia made use of the direct rays of the sun in his experiments: as this source of illumination, however, is not always available, and, even if so, attended with obvious inconveniences in practice, Czermak proposes the use of a perforated concave mirror of 7—12" focal distance, by which the light of an ordinary lamp can be concentrated upon the larynx-speculum, the eye of the observer being applied to the perforation. As the distinctness of the image will depend upon the brilliancy of the illumination employed, it will be found advantageous to concentrate the light of the lamp upon the concave mirror, by means of a powerful bi-convex lens. Dr. Levin, of this city, has devised a highly convenient apparatus for this purpose, consisting of a tin tube carrying a convex lens of two and a-half inches focal distance, and about the same diameter, which, by means of a

simple contrivance, can be fixed horizontally over an Argand lamp after the shade has been removed.

The perforated concave reflector can either be held between the teeth of the observer, fixed on a suitable ivory handle, as recommended by Czermak, or attached to a large spectacle-frame, according to Stellwag's proposal, or it can be suspended from a support screwed to the corner of the table on which the lamp is placed. The latter contrivance will be found the most convenient for practical purposes. I think it was first introduced by Dr. Levin (*a*).

It will be most convenient to place the lamp to the right of the patient, who is to be examined in the sitting posture, his hands resting upon his knees, his body slightly advanced, and his head slightly reclining backwards. According to Professor Traube's advice, the lamp, concave mirror, and larynx-speculum ought to be on the same level, and the angle formed by the rays incident upon, and reflected from, the concave mirror as acute as possible. On this account it will be wise to place the lamp a little behind the patient. The observer supports the head and chin of the patient with his left, and introduces the larynx-speculum with his right hand, looking through the perforation of the concave mirror, by means of which he illuminates the pharynx.

By causing the patient to sound alternately the Roman vowels, *a. e.*, the velum and uvula will be raised so as to admit of the mirror being introduced with greater facility. In pressing the speculum against the soft palate and uvula, great care must be taken to avoid touching the posterior wall of the pharynx, the palatine arches, and the base of the tongue to prevent the supervention of vomiting and deglutition. "In this manner," as Czermak says, "it is possible to look into the very depths of the pharynx, to obtain a distinct image of the individual parts of the larynx, and, as I first demonstrated in my own person, to see the bifurcation of the trachea reflected through the widely opened glottis, with the tracheal rings shining through the thin mucous membrane."

Of course, considerable practice and a certain amount of dexterity are required for successful handling of the laryngoscope, notwithstanding the simplicity of the principle upon which the method is founded.

The difficulties are mainly owing to the great irritability of the palate, which, in some individuals, is so considerable as not to tolerate the contact of a foreign body; others are unable to

(*a*) Mr. Yearsley has requested us to state that he has used Mr. Avery's ear-lamp in this way for several years past.—Ed.

keep their mouths open for any length of time, or to command the position of the tongue, which ought to be well flattened and protruded. Some patients, as Professor Traube correctly remarks, suffer from a kind of "moral nausea," threatening to vomit as soon as they are told to open their mouths. This extreme irritability can be overcome by methodically accustoming the parts to the contact of foreign bodies, as it is often requisite prior to surgical operations on the palate. I remember reading that bromide of potash has the power of lowering the sensibility of the pharyngeal mucous membrane; it might deserve a trial in very refractory cases.

In general, however, the irritability of uvula and soft palate will be found very inconsiderable, so that they can be raised and pressed against the posterior wall of the pharynx without any inconvenience to the individual experimented upon. In Professor Traube's clinic I have seen an individual sitting for nearly ten minutes with the larynx-speculum applied to the fauces, so that fifteen medical men who were present could successively examine the reflected image of the glottis without any reflex phenomena supervening to interrupt the observations.

In this case the mouth of the patient was held open by a very convenient instrument, devised by Dr. Levin. The handle of the larynx-mirror is attached by a ball-hinge to the upper bar of the mouth-speculum, so as to admit of the larynx-mirror being easily adjusted for the purpose of demonstration.

In the fifth chapter, Czermak details his method for obtaining a view of the posterior surface of the velum, the naso-pharyngeal cavity, etc. and he represents the image obtainable by rhinoscopic investigation, the commencement of the Eustachian tubes being also rendered visible. Wilde has already investigated the latter by a similar method.

To obtain an image of these parts, a speculum must be introduced under the velum, with its reflecting surface turned obliquely upwards, so as to illuminate the naso-pharyngeal cavity. A speculum is proposed for this purpose, to which a sliding wire-hook is attached, for the purpose of raising the velum.

Examinations of this kind are, of course, surrounded by numerous difficulties, and can only be expected to succeed if a combination of favorable circumstances obtains.

The auto-laryngoscopic observations instituted by Czermak for physiological purposes are mainly confirmative of the results obtained by Garcia's celebrated investigations, and his work will amply repay perusal to those who are interested in

the important questions involved in the study of the mechanism of the human voice.

The pathological observations which conclude the work, twenty in number, illustrating most varied and interesting forms of laryngeal disease, as revealed by the larynx-speculum, are calculated to convince the most sceptical of the great advantages which must accrue to the practitioner from the adoption of this method of investigation.

The possibility of the eye serving as a guide for the hand in the topical treatment of affections of the larynx and deep parts of the pharynx, is also proved by some of these observations. You must permit me to reserve my detailed statement for a future communication. Two of these cases—the first and third—during the course of which laryngotomy had to be performed, on account of stenosis of the larynx, are of particular interest, being the first in which, by a novel adaptation of laryngoscopy, the glottis was investigated from below. This was effected by introducing a small mirror attached to a suitably bent handle, with its reflecting surface turned upwards, into a fenestrated tracheotomy-tube. By illuminating this speculum with a concave reflector, the most brilliant and accurate images of the lower aspect of the glottis, etc., were obtained, and the nature of the pathological changes affecting the parts clearly ascertained. This method promises to be of great importance for the diagnosis and treatment of deep-seated affections of the larynx, particularly in cases of laryngeal tumors which cannot be attacked from above. By reversing the reflecting surface of the mirror introduced into the tracheotomy-tube, the deep parts of the trachea might also be explored.

MEDICAL CONVENTIONS.

I. *Medical Teachers' Convention.*

NEW HAVEN, JUNE 4, 1860.

The Convention of Medical Teachers, according to adjournment at Louisville, Ky., met at 10 o'clock, A. M., in the lecture-room of Yale Medical College, Dr. Dixi Crosby, President, in the chair, and, on motion, adjourned to meet again at 3½ o'clock, P. M.

AFTERNOON SESSION.

The Convention met according to adjournment, and was called to order by the President. The Secretary being absent, on motion of Prof. Palmer, of Michigan, Prof. H. A. Johnson was elected Secretary.

The minutes of the Convention at Louisville were read, and, on motion, a list of the delegates was prepared, from which it appeared that the following institutions were represented: The Long Island Hospital, by Prof. Austin Flint; Medical Department of Dartmouth College, by Prof. Dixi Crosby and O. P. Hubbard; Medical Department of the University of Louisville, Ky., by Prof. R. I. Breckenridge; Savannah Medical College of Georgia, by Prof. R. O. Arnold; Medical Department of Yale College, Conn., by Profs. Jonathan Knight and Benj. Silliman, Jr.; Medical Department of the University of Michigan, by Prof. A. B. Palmer; Harvard Medical College of Massachusetts, by Profs. D. Humphreys Storer and G. C. Shattuck; Berkshire Medical College of Massachusetts, by Prof. William H. Thayer; Medical College of Virginia, by Prof. J. B. M'Caw; Atlanta Medical College of Georgia, by Prof. Jas. P. Logan; Missouri Medical College, by Prof. Jos. N. M'Dowell; Medical Department of Lind University, Ind., by Profs. N. S. Davis and H. A. Johnson; Medical College of South Carolina, by Prof. Henry R. Frost; Iowa University, by Profs. D. L. McGugin and Daniel Meeker; Geneva Medical College, by Prof. Frederick Hyde; Albany Medical College, by Prof. Alden March.

The committee appointed at the previous Convention to con-

fer with a similar committee of the American Medical Association, reported through their chairman, Prof. Shattuck, a preamble giving an account of their doings, and proposing a series of resolutions, as follows:

1. *Resolved*, That the Medical Colleges represented in this Convention are willing to adopt the rule, if it be recommended by the American Medical Association, that every candidate for the degree of Doctor of Medicine must present certificates of having assiduously studied medicine during the period of three full years under the direction of a regular practitioner of medicine, recognized as such by the American Medical Association, who shall certify to the same under his own hand, and of attendance on two *full* courses of medical lectures in a medical school, recognized as regularly organized by the American Medical Association, with an interval of at least three months between the termination of the first course and the commencement of the last.

2. *Resolved*, That the Medical Colleges represented in this Convention are willing to keep a register of their students, in which shall be entered the name, the age, the period of commencing medical studies, and diploma already received, with the name of the college conferring it, and the name of the preceptor.

3. *Resolved*, That the Medical Colleges represented in this Convention, allowing that the proposed plan of admitting delegates from State Societies to attend the examination of the candidates for the degree of Doctor in Medicine to have been successfully carried out in several places, do not think that it can with advantage be universally adopted; but, at the same time, they are ready to ascertain and discuss any other measure by which the admission of unsuitable and unworthy members within the ranks of the profession can be prevented.

4. *Resolved*, That this Convention earnestly recommend the American Medical Association to adopt such measures as will secure the efficient practical enforcement of the standard of preliminary education adopted at its first organization, in May, 1847, or of a standard put forth by the Medical Society of the State in which a college is located, and that Medical Colleges will thankfully receive and record the certificates alluded to in said standard and one of moral character, whenever the profession generally and the preceptors will see that students are properly supplied with them.

5. *Resolved*, That Hospital Clinical Instruction constitutes a necessary part of medical education, and that every candidate for the degree of Doctor in Medicine shall be required to have attended such instruction regularly for a period of not less than four months.

6. *Resolved*, That the members of this Convention are ready to co-operate in any efforts by which the attention of the community and of legislatures shall be called to the importance of the endowment of Medical Colleges and Professorships.

7. *Resolved*, That the attention of the American Medical Association be called to the proofs, in a letter from a German medical pro-

fessor, of the degree of Doctor in Medicine being conferred in Germany on unsuitable persons to be used in this country.

On motion of Prof. Davis, the report was received, and the resolutions taken up seriatim.

Prof. Flint moved to amend the first resolution, by omitting the words "with at least an interval of three months between the termination of the first and the commencement of the last."

The amendment was discussed somewhat at length by Profs. Flint, M'Dowell, Davis, Palmer, Shattuck, Arnold, Frost and Logan; after which it was rejected.

On motion of Prof. M'Dowell, the first resolution was laid on the table, to be taken up at a future time.

On motion of Prof. Thayer, the second resolution was adopted.

The third resolution was discussed by Profs. M'Caw, Breckenridge, Knight, Palmer, M'Dowell and Davis.

Prof. Logan offered the following as a substitute for the whole report:

Whereas, It is apparent that the Medical Colleges of the United States are not disposed to adopt the measures indicated by the American Medical Association, for the establishment of a higher system of medical education, as manifested by the failure upon the part of a large portion (and among the number some of the most prominent) to be represented at the Convention of Colleges, held last year in Louisville, and by a renewal of the same course of action towards the adjourned meeting of said Convention, and as no action on the part of the Colleges represented would be likely to effect any change in the present system of medical education, and any attempt on the part of this limited representation to initiate any reform might be regarded as an offensive assumption of power: Therefore,

Resolved, That this body declines to act for the Medical Colleges of the United States.

Resolved, That in the Medical Colleges alone resides the power of effecting any desirable change in the present system of medical education, and it is only from their united action than any good result can be expected.

Resolved, That a Committee of — may be appointed to report the action of this body to the American Medical Association.

The substitute was discussed by Profs. Logan, Shattuck, Crosby, M'Gugin, M'Dowell, Storer and Palmer, and was finally rejected.

At this stage of the proceedings, Prof. Logan, of Georgia, retired from the Convention, stating that he did not feel at liberty to act with it as the representative of the Atlanta Medical College.

On motion, the Convention adjourned till Tuesday morning, at 9 o'clock.

SECOND DAY'S PROCEEDINGS.

The Convention was called to order by the President, Dr. Crosby.

The following additional Institutions were represented: University of Maryland, by Prof. Edward Warren; University of Buffalo, by Profs. Thomas F. Rochester and James P. White; St. Louis Medical College, by Prof. I. B. Johnson; Castleton Medical College, by Prof. E. K. Sanborn; Maine Medical College, by Prof. Nourse.

On motion of Professor Shattuck, the third resolution was adopted.

On motion of Professor M'Dowell, the fourth resolution was adopted.

On motion, the order of business was suspended, when Professor Frost presented the following communication in regard to Medical Education in the South:

"I should wish to be heard while I make a few remarks on the progress of Education at the South, and the advances we have made in fulfilling the requirements of the Association. The report in my hand of the Dean of the Medical College of the State of South Carolina, of the graduates of that College and their requirements, presents a total of 114 graduates—all of whom had a preparatory education, such as the Association requires. Nearly all, with the exception of six, have had good literary opportunities: some graduates of colleges, others of academies of high repute, others instructed in the classics. Even those whose studies were confined to English, have had their minds strengthened by the study of mathematics.

In making this statement, I would not be understood to say that they were well versed in the classics; but they have enjoyed the opportunity and profited in a greater or less degree by it. Neither would I be understood to say that our graduates are all doctors. The diploma conferred is only an evidence that they have undergone a course of study; that they have been instructed in the principles of the profession, and made acquainted with the means by which they are to arrange and systematize the various occurrences presented to them—in short, that the foundation has only been laid by which they are to peruse advantageously their researches, and act for themselves. To be able doctors and successful practitioners, requires years of study and observation, and there

are many who after all this application have never been made doctors.

The community in which a young graduate resides, soon becomes aware of this fact; it is only after a long apprenticeship, and years of toil and devotion to his business, that he acquires practice and confidence. Confidence is proverbially a plant of slow growth, and it is only after the individual has proved himself worthy that it is freely bestowed. Still, however, every doctor has been a student, and as such, has to endure taunts and imputations as to his qualifications. I well remember when a student in medicine, forty-seven years since, fashionable ladies commented upon the homely appearance and neglected dress of the students of Philadelphia, and tauntingly observed that there was little to be observed in the streets but dogs and Virginia doctors! Yet from these classes of whom these remarks were made, there came forth a Wood, Mitchell, Meigs, McClelland, Hodge, Bartons, Derach, and not to forget my own section, Dickson, Holbrook, Ramsay, and many others. Yet these young men were as ungainly as many of the present day; but they contained the gem, as many of the present day, which required only to be polished. Education has been progressive to my observation; our graduates show their desire to excel by seeking opportunities abroad for greater acquirements. In my day our reading was desultory and without system. My preceptor pointed to his library and told me to select my reading. My anatomical studies were pursued with the scalpel and the Dublin dissector. Our clinical instruction was nothing virtually. Mark the difference at the present time. Your winter and summer courses; your crowded hospitals; your private instructions, and your model plates, &c. All these speak trumpet-tongued that the work of improvement is onward."

On motion of Professor McDowell it was directed to be appended to transactions of this body, for the American Medical Association.

On motion the fifth resolution was adopted.

On motion of Prof. McDowell, the sixth resolution was adopted.

On motion of Prof. Shattuck, the seventh resolution was adopted.

On motion of Prof. Arnold, the first resolution was taken from the table.

Prof. Shattuck offered for the first resolution a new one precisely the same as the first, with the exception of the last clause in regard to the interval of time between the first and last courses of lectures.

It was discussed by Profs. Shattuck, McDowell, Flint, Arnold, Breckenridge, Davis, Palmer, McCaw, Nourse and White.

Prof. White moved that the substitute and the original resolution be laid on the table. The motion was lost.

Prof. Breckenridge called for the vote on the substitute offered by Prof. Shattuck by colleges.

The substitute was lost by the following vote:

Ayes—Long Island College Hospital, Medical Department of Dartmouth College, Medical Department of the University of Michigan, Berkshire Medical College, Iowa University, Castleton Medical College, University of Buffalo, Maine Medical College—8.

Noes—Medical Department of the University of Louisville, Savannah Medical College, Medical Department of Yale College, Harvard Medical College, Medical College of Virginia, Missouri Medical College, Medical Department of Lind University, Medical College of the State of South Carolina, Geneva Medical College, Albany Medical College, University of Maryland, St. Louis Medical College.—12

Profs. McGugin and Palmer, in voting for the substitute, explained that they did so because they were in favor of the propositions therein contained, and hoped that a distinct proposition, relating to the length of the *interregnum* of courses similar to that contained in the original resolution might be presented, that they might vote for it.

The motion on the original resolution was then taken by colleges, and adopted by the following vote:

Ayes—Medical Department of Dartmouth College, Savannah Medical College, Harvard Medical College, Berkshire Medical College, Medical College of Virginia, Missouri Medical College, Medical Department of Lind University, Medical College of the State of South Carolina, Iowa University, Geneva Medical College, Albany Medical College, University of Maryland, St. Louis Medical College, Castleton Medical College—14.

Noes—Long Island College Hospital, University of Buffalo, Maine Medical College—3.

On motion of Prof. Davis—

Resolved, That the committee of which Dr. Shattuck is chairman be requested to report the doings of the Convention, with the resolutions adopted to the American Medical Association.

On motion, the Convention adjourned to meet again at the call of the President.

H. A. JOHNSON, Secretary.

2. *Thirteenth Annual Meeting of the American Medical Association.*

FIRST DAY.

The Convention met in the College Chapel, and at 11 o'clock was called to order—President Dr. Henry Miller, of Kentucky, in the Chair.

Prof. Fisher, of Yale College, opened the Convention with prayer.

Dr. Chas. Hooker moved the Committee of Reception report. Dr. Knight, as chairman of the committee, made a most felicitous salutatory to the Convention, and was received with considerable applause. He spoke ably, and showed the lofty character of such a Convention. His remarks on the advance in remedial agents, and the progress of medicine, and especially of the art of surgery, were received with marked attention. He reviewed with his remarkable liveliness and interesting manner, the more wonderful improvements, such as the ligature of the greater arteries; the introduction of anæsthetic agents in the greater operations, thus relieving that stinging pain and great anguish to which patients were formerly subjected. He closed by welcoming the profession here, describing most graphically our city attractions by way of literary institutions, &c., and welcoming them all to our open homes and hearts.

Dr. Chas. Hooker then followed in a welcoming address, and was frequently applauded. He spoke as follows:

MR. PRESIDENT AND GENTLEMEN OF THE AMERICAN MEDICAL ASSOCIATION:—It is with unwonted gratification that the Committee of Arrangements welcome you to the city of New Haven. And we only bespeak the common feeling of our fellow-citizens in saying that we are delighted—nay, proud—to receive you as our guests. We feel that any city is highly honored to become the chosen place of meeting of the American Medical Association—a select delegated national Congress, representative of forty thousand members of a learned and humane profession. As a city, we appreciate this honor, and should be ungrateful did we not receive you with a generous and cordial welcome. You meet, gentlemen, for a great and noble object—for the promotion of a science vitally linked with the interests of humanity. Your meetings have a most happy influence in strengthening those ties by which the great fraternity of medicine are bound in social compact. Another salutary incidental benefit of your meetings, results from their affording an annual period for relaxation and social enjoyment.

Too many physicians prematurely break down in their career of usefulness, in consequence of unremitted and arduous application to

their professional duties; and many of you now present, whose exhausted physical and mental energies need recruiting, could hardly have been drawn away from your routine of toil and care, but for your sense of bounden duty to aid in the great object of this Association. We congratulate you, therefore, brethren, on this annual recurrence of our national medical jubilee. In behalf of the Faculty of Yale, we welcome you to the halls of this ancient seat of learning, in which you are invited to hold your sessions; and in behalf of the citizens generally of New Haven, we tender you the hospitalities of our city.

We hope that to all of you this meeting will be a season of pleasant social intercourse long to be remembered for the many friendships here formed; and we trust that the harmony and wisdom of your counsels will efficiently promote the great benevolent objects of our organization."

The President ordered as the next business, the calling of the roll. This occupied some half an hour, which the galleries and the Convention to some extent used as a recess.

The whole number of delegates who answered to their names was between 275 and 300. When all are present the whole number of delegates will probably exceed the last figure. Twenty-seven States and the District of Columbia are represented in the Convention. There are also a few from the United States Navy.

Dr. Charles Hooker here gave notice of the five divisions and their respective rooms, as follows:

1st, Anatomy and Physiology, President's Lecture Room.

2d, Surgery, Geological Cabinet.

3d, General Medicine, Geological Cabinet.

4th, Chemistry and Materia Medica, Chemical Laboratory.

5th, Meteorology, Chemical Laboratory.

Dr. James Hadley, of Mass., moved that if any surgeons or physicians from the navy be present, they be invited to take seats on the floor of the Convention; carried.

Dr. John Bronson asked if seats had been reserved for the ladies attending the Convention with their husbands; also, for ladies in general.

Dr. Charles Hooker stated that a committee was formed from the State and city medical societies to make themselves useful among the ladies,—also that the galleries of the college chapel would be open every morning at 8 o'clock, and the delegates could be present at college prayers who so wished. The President stated that the committee on parliamentary rules were ready to report; ordered.

Report was read.

Dr. Brodie moved, before the resolutions be acted on, they be printed.

Dr. Cox moved an amendment, that 500 copies be printed. Amended again by the motion that 1,000 copies be printed.

Here an exciting discussion took place in regard to the necessity of having them printed, merely for acting upon, and after an indiscriminate debate, urging the prevalence of pet motions and amendments, a motion to lay the whole affair on the table prevailed by a small majority.

A motion for a recess of ten minutes was then carried, the object being to give each State an opportunity to choose its member of the nominating committee.

At a quarter before one o'clock, the Convention re-assembled, when the nominating committee was declared.

A motion was made and carried, inviting the Legislature to be present at the opening of the Convention in the afternoon to listen to the address of the President, as it would have some reference to medico-legal topics.

AFTERNOON SESSION.

At 3 o'clock the Convention came together, and, notwithstanding the unpleasant weather, the galleries were well filled, including quite a number of ladies.

Convention called to order.

Gov. Buckingham and Lt. Gov. Catlin appeared on the stage, and were introduced to the Convention amidst applause.

The Secretary, Dr. Bemis, of Kentucky, then gave the names of the Committee on Credentials.

When the House had become still, President Henry Miller was introduced, and delivered his Valedictory Address. Quite a number of the members of the Legislature were present. Most of it was a bold exposition of personal opinions regarding the moderate and limited standard of medical education. We could see, as he advanced his views regarding preliminary instruction and the duties of Medical Colleges in raising their standards of requirements, that he had the cordial support of the Convention by the earnest attention and frequent applause attending his suggestions.

The Nominating Committee here reported the names of officers for the Convention, as follows:

President—Eli Ives, Conn.

Vice Presidents—Wilson Jewell, Pa.; A. P. Palmer, Mich.; Joseph P. Logan, Ga.; I. N. McDowell, Mo.

Secretaries—Not reported.

Treasurer—Caspar Wistar, Pa.

The various Committees were then appointed to wait upon the different grades of officers to the stage.

Several invitations to visit prominent public places and factories of the city were read and times set apart for such visits. At 5 o'clock this (Wednesday) afternoon, the Convention will visit Messrs. G. & D. Cook & Co.'s carriage factory.

Motions were made of acceptance of the invitations, &c.

Motion made to suspend business and receive the officers just elected. They were received with great and prolonged applause.

President Dr. Ives made a very short address, of which the following is nearly a verbatim report :

"All he had, all he was, he owed to his profession. He loved it. He had two sons in the profession, also a grandson; and he, like a very distinguished physician of the present century, could say he would visit the sick as long as he could go, and when he was unable, he would be carried to the bedside."

He was followed by first Vice-President, Dr. Wilson Jewell, who will preside over the deliberations of this Convention.

Dr. Davis, of Indiana, offered a series of resolutions to the specific business of morning and afternoon sessions, as follows :

Resolved, That the general meetings of the association after this day, shall be restricted to the morning sessions, and the afternoon sessions, commencing at 3 o'clock, shall be devoted to the hearing of papers and discussions in the several sections.

Resolved, That each section shall choose its own officers and make its own rules of order.

There were other resolutions of this same series which are not reported, because not finally acted upon. The third one, relative to the referring of public essays, addresses, &c., to their respective sections, caused a long and exciting debate, in which Drs. Watson, Reese, Miller and Palmer figured conspicuously. After the discussion had run over an hour, without seeming to come to any mutual understanding in the matter, a motion to table this section of the resolutions was almost unanimously carried, with the proviso that Dr. Davis should have an opportunity to revise it, and at his own time to bring it again before the Convention.

Dr. Little, of California, was then announced, and, although not a regular delegate, was invited to a seat on the floor of the Convention, there being no delegate from California.

A committee on Voluntary Communications was then appointed, viz : Drs. E. D. Force, of Kentucky; T. W. Blatchford, of New York; N. S. Davis, of Illinois; R. LaRoche, of Pennsylvania; Rochester, of New York.

At his own request, Dr. LaRoche was excused from serving on this committee.

Dr. Reuchberger, of Pennsylvania, was appointed in his stead.

The report of the Treasurer was then called for, read and adopted, then referred to Committee on Publication.

The Committee on Publication then reported. Report accepted.

Committee on Prize Essays was called on to report, but failed to do so through absence.

Adjourned.

SECOND DAY—WEDNESDAY.

The Convention was called to order by the first Vice-President, Dr. Wilson Jewell, of Penn.

The minutes of the Convention of yesterday were read and approved.

The President announced that the subscription list for the publication of the Sydenham Society was on the Secretary's table.

An opportunity was now given for delegates to name physicians from States not represented, also from the Army and Navy, as members by invitation.

Dr. Gardner moved that the rules of order be suspended for Dr. Logan, of Ga., to tender his resignation as Vice-President. Resignation accepted.

Committee on Education reported—Dr. Reese chairman.

This was far the most lengthy and deep-studied report yet made, being a most able exposition of the necessities of our Medical Colleges. We hope to give this in full hereafter. He particularly dwelt on the necessities in preliminary education—Practical Anatomy, Pathology and Clinical Medicine. He ably supported his argument in favor of lengthened terms of study, with a less number of lectures per day—four being the maximum.

Dr. Bodie moved that the report and resolutions connected with it be received and referred to the Committee on Publication. Received.

On motion, the House went into a Committee of the Whole on the resolutions—H. F. Askew, M. D., in the Chair.

Dr. McDowell, of Mo., spoke against the first resolution, and immediately the galleries were densely crowded, and every effort made to get a sight of the witty and wiry Missourian. Almost every sentence drew forth roars of laughter. He was loudly cheered and often interrupted by the repetition of applause.

Dr. Henry Miller, of Ky., replied.

Dr. Palmer, of Mich., continued the discussion.

Motion made that the whole subject be laid on the table.

Motion made and carried that the committee rise, report progress, and sit again.

Committee on Medical Literature called upon.

The Committee on Nominations reported that the Convention will meet at Chicago on the 1st Tuesday in June, 1861. Amendment offered that it be changed to the 1st Tuesday in May.

Dr. Davis, of Illinois, spoke for the Illinois delegation, urging June as the proper month—furthermore, he welcomed the Convention to the hospitalities of the citizens of Chicago.

Motion made to change the time to the 2d Tuesday of June. Unconstitutional.

The whole list of officers was not reported yesterday. The Committee on Nominations here concluded their report, as follows :

In place of third Vice-President, Dr. Logan, of Ga., resigned, Dr. R. D. Arnold, of Georgia.

Secretaries.—S. G. Hubbard, Conn. ; H. A. Johnson, Ill.

Committee of Arrangements.—N. S. Davis, G. W. Freer, Dr. Laski Miller, E. Andrews, H. W. Jones, Thomas Bevan, J. Bloodgood, all of Illinois.

Prize Essays.—Daniel Brainard, Ill. ; D. L. McGugin, Iowa ; M. L. Seaton, Mo. ; John Evans, Ill. ; A. S. McArthur, Ill.

Committee on Publication.—S. G. Smith, Penn. ; Caspar Wistar, Penn. ; S. G. Hubbard, Conn. ; R. I. Breckenridge, Ky. ; Ed. Hartshorne, Penn. ; H. F. Askew, Del.

Dr. Davis, of Ill., called for a suspension of the rules, that he might re-introduce his resolution laid upon the table yesterday. Carried. The resolution having been revised, was reported and carried.

Report of Committee on Prize Essays was called for. Prof. Worthington Hooker, of Conn., Chairman. Three essays had been handed in—two of which had considerable merit, and showed much research. The Committee had concluded not to award any prizes this year. Report accepted.

Moved a suspension of the rules to give Dr. Wilbur, of New York, an opportunity to report the protest of Dr. Ignatius Langer, of Iowa, against the action of the Committee of Arrangements in not accepting his credentials as a delegate. The President stated he held in his hand a letter stating that Dr. Langer had been expelled from the Scott County Medical Society of Iowa, and, therefore, the rules of the Society would not permit his acceptance as a delegate here.

Motion to suspend lost, almost unanimously.

Reports of Special Committees were then called for and disposed of in various ways.

One o'clock, the hour of adjournment, having arrived, a motion to continue five minutes longer prevailed. A little general business was then transacted, and the Convention adjourned.

AFTERNOON SESSION.

The Convention was called to order by the Chairman at 3 o'clock.

According to the resolution carried the day previous, the Convention adjourned to the various sections as follows:

Anatomy and Physiology—President Woolsey's Lecture Room.

Chemistry and Materia Medica—Chemical Laboratory.

Practical Medicine and Obstetrics—Geological Cabinet.

Surgery—Geological Cabinet.

Meteorology—Chemical Laboratory.

THIRD DAY.

The Association was called to order at 9 A. M., by the President, Dr. Eli Ives; afterwards, Dr. Jewell, of Philadelphia, presided.

The minutes of the previous days' proceedings were read by the first secretary, Dr. S. G. HUBBARD, of New Haven.

A list of newly registered delegates was read, making the number over five hundred.

On motion of Dr. Arnold, of Georgia, it was resolved that no communication read before the Association should occupy more than ten minutes in its reading, and no speaker should occupy the floor longer than ten minutes.

On motion of Dr. Shattuck, of Massachusetts, the rules of order were suspended, in order to allow Dr. Bowditch, Chairman of the Committee appointed to take into consideration the propriety of contributing in the erection of a suitable memorial to John Hunter, in Westminster Abbey, to present his report. On motion, it was resolved that the Committee on Nomination be requested to consider the report and resolutions attached to it, and report thereupon, presenting the names of of one from each State represented, who shall be empowered to take such action in the matter as may be hereafter agreed upon by the Association.

The Committee of Conference appointed to confer with the

Committee of Medical Teachers reported through their chairman that they had had several meetings in New York and New Haven during which the subject of medical education had been fully discussed.

The Committee offered the following resolutions for adoption by the Association :

Resolved, That it is the duty of medical colleges to require of every candidate for the degree of Doctor of Medicine, certificates of study during the full period of three years, under the direction of a regular practitioner of medicine, recognized by the American Medical Association, who shall certify, under his own hand, as to an attendance on two full courses of lectures, with an interval of at least three months between the termination of the first and the commencement of the second course.

Resolved, That every medical college shall keep a volume, in which every medical student presenting himself, shall enter his name, his age, the period of his commencing the study of medicine, any diploma he may have received in evidence of previous education, with the name of the college or school from which he received such diploma ; and the name of the preceptor with whom he has been studying.

Resolved, That hospital clinical instruction constitutes a necessary part of medical education, and every candidate should be required to have attended such instruction regularly for a period of not less than four months.

Resolved, That the professors of every medical college should recommend to their trustees, or board of managers, the adoption of a rule authorizing them to allow the attendance of two or three delegates, from the State Medical Society, at all examinations of candidates for the degree of the doctorate, and accord to these delegates a vote on the question of recommending such candidates for a degree.

Resolved, That every State Society be recommended to choose proper delegates at its annual meeting, to attend the examination of candidates for the degree of M. D., at all the medical colleges within their respective States.

Resolved, That this Association will not recognize as a regular organization; any college which does not require evidence of suitable preliminary education from all applicants for collegiate medical instruction.

Resolved, That we commend the use of all proper efforts, by which the attention of persons of means and liberal disposition, as well as legislative bodies, shall be directed to the propriety of endowing such medical colleges, and professorships thereof, as shall be recognized by the association.

Resolved, That this Association recognize as a regularly organized medical college, one which has been represented at any meeting of this Association, and which complies with the preceding rules and directions.

Resolved, That this Association recognize as regular practitioners of

medicine, all who have been members of this Association, and have not forfeited their rights and privileges, and all members of State and county Societies, in full standing.

The report was received, and taken up by sections. When the first resolution came up, a motion was made to amend, by striking out that part requiring an interval of three months to elapse between the termination of the first course and the commencement of the second; the objection being that the resolution, if adopted as offered, would do an injustice to summer schools, whose sessions would have to begin three months after the closure of the winter sessions, in order to graduate students, thus throwing the session into July, August and September, and crowding upon the next winter session; and that such a course would drive students altogether from the summer schools.

Dr. McDowell, of Missouri, spoke in strong terms against the amendment. He despised the plan of some professors, who teaching at a winter school in the South, immediately the winter session closes, bring their half fledged brood to a Northern summer school, and there delivering a second course of lectures, foist their hastily hatched students upon the medical profession. He was entirely opposed to the practice of pushing and forcing, which was becoming so rampant.

The discussion was further participated in by Drs. Shattuck, of Boston, Austin Flint, N. Y., Brodie, of Mich., Palmer, of Mich., Morse, of Me., Atlee, of Pa., McCaw, of Virginia, and others, and the resolutions were finally adopted and referred to the Committee on Publication, for publication in the forthcoming volume of Transactions.

The fifth resolution gave rise to a good deal of discussion as to the propriety and the right of placing medical schools under the censorship of the State Medical Societies.

Dr. Timothy Childs, of Berkshire, Mass., stated, that forty years ago he called for a board of examiners to be present at all examinations for a degree, and that he had never ceased to urge the propriety of so doing. He had never passed a student without such a supervision.

He stated that he was the first man to introduce into medical colleges a Professorship on Pathology, and he was always in favor of enhancing the dignity and worth of his profession, and as long as he was able to raise his voice, he would oppose to the utmost all those who attempt to lower the standard of medical excellence, regardless of the motives that prompt them to do so.

Dr. Worthington Hooker, of New Haven, Ct., explained

that Yale College, further back than forty years ago, had, of its own accord, adopted the plan contained in the resolution under consideration, and during his connection with the College, there had not been one whisper of disapprobation regarding it. There was harmony between the State Medical Society and the institution, which feels the genial effects of that harmony, which gives it its strength and position.

He thought that all medical colleges should be closely watched by the State Medical Societies of their respective States.

The Committee on Nomination then reported the following appointments on Standing and Special Committees, which was received and adopted, and the nominations accepted:

Committee on Medical Literature.—Frank H. Hamilton, N. Y., Chairman.

Committee on Medical Education.—L. S. Joynes, Va., Chairman.

On the Surgical Treatment of Strictures of the Urethra—James Bryan, Pa.

On Drainage and Sewerage of Large Cities—their influence on public health—A. J. Semmes, La.

On Puerperal Tetanus—its statistics, pathology and treatment—D. L. McGurgin, Iowa.

On Anæmia and Chlorosis—A. P. Ayres, Ind.

On Alcohol and its Relations to Man—J. W. Dunbar, Mn.

On Milk Sickness—Robert Thompson.

On Microscopic Observations on Cancer Cells—G. W. Norris, Pa.

On Blood Corpuseles—A. Sager, Mich.

On the Hygienic relations of air—C. C. Cox, Md.

On Quarantine—D. D. Clark, Pa.

On Medical Ethics—Paul F. Eve, Tenn.

On Tracheotomy in Membranous Croup—A. N. Dougherty, N. J.

On the effect of Perineal Operations for Urinary Calculi upon Procreation in the Male—J. S. White, Tenn.

On Mercurial Fumigations in Syphilis—D. W. Yandell, Ky.

On the Cause and Increase of Crime, and its Mode of Punishment—W. C. Sneed, Ky.

On the Microscope—R. C. Stiles, Vt.

On Gangrene of the Lungs—C. L. Allen, Vt.

On the Relations which Electricity sustains to the Courses of Disease—Isaac Capelbury, Ind.

On the Morbid and Therapeutic Effect of Verbal and Moral Influences—Alfred Hitchcock, Mass.

On the Causes of the Extinction of Aboriginal Races, more especially of the Red Men of America—Geo. Suckley, N. Y.

To report on the practical workings of the U. States law relating to the Inspection of Drugs and Medicines—E. R. Squibb, New York

On the Causes and Treatment of Ununited Fractures—E. K. Sanbone.

On Diptheria—Alonzo Clark, New York.

On the Effect of Stimulants in the Treatment of Fractures—John W. Russel, Ohio.

On Dislocation of the Hip and Shoulder Joints—Moses Gunn, Michigan.

To investigate the conditions demanded for a Diploma of Doctor of Medicine in the various Medical Schools and Universities of Europe—J. Baxter Upham, Mass.

In regard to the Committee on the Memorial to John Hunter, the following resolutions were adopted :

Resolved, That it be recommended to the different States to collect subscriptions of not more than one dollar each from every regularly educated physician. All money so collected to be forwarded by the Chairman of the Committee hereby appointed, to the Treasurer of the Hunter Medical Fund in London.

Resolved, That Drs. Henry J. Bowditch, Mass.; Charles Hooker, Conn.; Henry D. Bulkley, N. York; Wm. Elmer, N. J.; John L. Atlee, Penn.; C. C. Cox, Md; J. B. McCaw, Virginia; Cornelius Boyle, D. C.; James H. Dickson, N. C.; H. K. Frost, S. C.; John Nott, Ala.; R. J. Breckenridge, Ky., and others, be a committee to collect subscriptions.

A resolution was adopted to send a copy of the resolutions passed to each Medical school in the country.

The order of the day was suspended by consent, and the following resolution was offered by Dr. McCaw, and made a part of the Report of the Conference Committee:

Resolved, That this Association shall prepare a conspicuous seal to be appended to the diplomas of every Medical College, which sha'l comply with all the requirements of the foregoing resolutions—this seal to be withdrawn whenever there should be any failure on the part of such institution to carry out its provisions.

It was moved, that in order to expedite business without a session next day, the sections meet at 2½ P. M., and at 4 P. M. the Association again convene to close business and receive their reports.

CLOSING SESSION.

The Association was called to order at 4 P. M., by V. P. Wilson Jewell, in the chair.

Various special committees were called upon to report, and failing to do so were discharged—other reports which had been placed on the Secretary's table were, without reading, on

motion, referred to the Committee on Publication, with power to act.

The various sections were called upon for their reports, and the various papers respectively discussed by them were referred to the Committee on Publication.

The report of the Committee on Rules of Order, lying on the table, was then called for and read, and the order of business acted upon, and the articles severally adopted, and afterwards the whole report was laid on the table.

A communication was read from the Essex Co. Medical Society, of the State of New Jersey, containing the following preamble and resolution, for action upon by the Association:

Whereas, the indiscriminate sale of poisonous drugs at retail, is fraught with danger to the community, be it

Resolved, That in the opinion of this Association, it is the duty of the public authorities in the different States of the Union to pass prohibitory laws against the retailing of morphia, strychnine, prussic acid, etc., except on the written prescription of a regular practitioner of medicine, or on the personal application of a well-known citizen; and that a committee be appointed in the different States, to endeavor to carry into effect the spirit of the resolution.

The paper was received and the resolution adopted.

The report was referred to the Committee on Publication, with power to act.

On motion of Dr. Davis, of Illinois, it was decided that the committee called for be appointed at his leisure by the President of the Association.

On motion, Dr. Cox, of Maryland, was requested to present at the next meeting of the Association a paper on Necrology.

Dr. A. N. Dougherty, from the Committee on Tracheotomy, reported that from the mass of facts they had gathered with regard to the result of this operation, the proportion of successful operations was 1 in 3 4-10. The statistics of cases in this country, as far as ascertained, was 17 cures out of 58 cases.

Trousseau before 1844 had 212 cases, of which there were 40 cures and 132 deaths—after 1848, he had in 49 cases 48 deaths. From 1849 to 1858, he had at the Children's Hospital, at Paris, 466 cases—which resulted in 126 cures and 340 deaths. Another operator met with but 4 cures in 36 cases. Statistics of other operators were presented, and at the request of Dr. Dougherty, the report was referred back to the committee, with power to complete the report, and present the same at the next meeting of the Association.

Dr. Bell, of Brooklyn, offered the following resolution:

Whereas, some of the papers submitted to this Association require a longer period of time for their examination than the annual meetings will admit of; therefore, be it

Resolved, That the several sections have power to refer such papers to experts, who shall determine whether they are worthy of being referred to the Committee of Publication, for publication in the Transactions.

On motion, this was laid over to the next meeting of the Association.

A motion of Dr. Chapin, that all papers which had not been disposed of by the sections, should be referred by the Committee of Publication to experts, who should report back to them, whether the papers were worthy of publication in the Transactions, was laid on the table.

Various rules of order and amendments to the Constitution, which had laid over from previous meetings, were again indefinitely postponed.

A communication from the Clinton County Medical Society of Iowa, to which was appended a catalogue of the College, was read.

This communication charged the Western Reserve College with having exceeded its rights and privileges, in conferring the degree of the doctorate upon one Freeman Thompson, who had not come up to the requirements of their curriculum, who had not been examined by the professors in the presence of censors, and who had not been in attendance on lectures since the session of 1848-9 a single day. It stated that at one time the Western Reserve College acknowledged the truth of the above charge, and at another time denied it.

They called the attention of the Association to this case, and desired that the Western Reserve College be refused representation in the Association. Various papers were appended to the communication, substantiating the truth of the facts mentioned.

A motion was made to refer the whole subject to a select committee of three, to be appointed hereafter by the Chairman, who should report on the same at the next annual meeting of the Association.

Dr. Davis, of Illinois, reminding the mover of the existence of a permanent Committee on Medical Ethics, created for just such purposes, the motion was altered to refer the matter to the Committee on Medical Ethics, with instructions to report at the next annual meeting, and carried.

A communication was read from the Legislature of Connecticut, stating that the Judiciary Committee had under consideration their memorial on criminal abortion, and asking, in

order to further the matter, that a committee be appointed by the Association, to frame a bill meeting the exigencies of the case, to be presented for due consideration of the Legislature.

It was moved and carried, that the chair appoint a proper committee, to draw up such a bill as would meet the views of the Association, and present the same to the Legislature of the State.

A motion was made to alter the time of meeting from June to May, so that if the Association desire to meet in 1862 in New Orleans, they could do so before the time when yellow fever occurs. This being an amendment to the constitution, was laid over for one year.

On motion of Dr. S. W. Butler, of Philadelphia, it was resolved that this Association request the Convention of Medical Teachers to be perpetuated in connection with the American Medical Association, and meet in conference the day previous to the annual meetings of the Association, and report to the same.

On motion, the same committee appointed last year was continued, any vacancies occurring to be filled by the President.

On motion of Dr. J. L. Atlee, of Philadelphia, the chairman of the committee on the memorial to John Hunter, was empowered to fill any vacancy which may occur in that committee.

A motion by Dr. Mason, of New York, that a committee of five be appointed to prepare rules of order for the Association, and to report them at the next annual meeting, was laid on the table.

A communication from Elmira, N. Y., was read, advising the offer of a prize for the best essay on the application of mechanical contrivances in the practice of surgery, having reference to the cure or alleviation of hernia, stricture of the urethra, stone in the bladder, fractures, dislocations, &c., was referred to the Surgical Section of next year.

A vote of thanks was passed to the retiring officers, for the efficient manner in which they had performed their duties.

A resolution was passed to the effect that the thanks of the Association are due to the Faculty of Yale College, the medical profession, and citizens of New Haven, for the elegant hospitality tendered to the Association; and to the proprietors of the different manufactories, for the generous manner in which they welcomed the delegation to inspect whatever of interest their factories embraced; to the railroad and steamboat companies, who have reduced their fare on the respective routes, in favor of the delegates to the Association.

Various amendments to the constitution, laid over from last year, were called up and indefinitely postponed.

Dr. Lewis A. Sayre, of New York, offered a resolution that the Smithsonian Institute be asked to collect all the medical literature that has appeared in this country, and is scattered in various journals and periodicals, and collect it in a general library for the purposes of the profession.

On motion of Dr. Davis, of Ill., the Association went into a committee of the whole to consider the report of the Committee on Medical Education, Dr. Askew, of Delaware, in the chair. An animated discussion ensued as to the extent of preparatory qualification, which ought to be exacted from young men designing to commence the study of medicine, but no conclusion being arrived at, the committee rose, and reported that they had considered the above report, but had no suggestions to make to the Association, and recommended the resolutions to the Committee on Publication.

Dr. Hamilton, of Brooklyn, N. Y., moved the adoption of a resolution to devise a plan for the organization of a College, or Board of Examiners, to be called the College of Physicians and Surgeons of the American Medical Association, in order to arrest all legislation which has reference to medical schools, and to determine what shall be the pre-requisites to a degree of doctor in medicine. Said College to consist of one member from each State, and to meet annually, immediately before the annual meetings of the Association.

Dr. S. W. Butler, of Philadelphia, stated that the whole plan in detail, only under a different name, had been brought before the Association at a previous meeting.

Dr. Cox, of Maryland, was exceedingly surprised at the idea of such a suggestion, and spoke against it in bitter terms, though at the same time he urged the necessity of a proper preliminary education for medical students.

Dr. Thompson, of Ohio, said that the asserted pre-requisites for a degree in reference to preliminary education, established twenty years ago, were always disregarded in his State.

After some general discussion on this subject, the Association, on motion, adjourned *sine die*.

3. *National Sanitary Convention.*

The fourth annual meeting of this Convention began at the Hall of the Charitable Mechanics' Institute, Bedford street,

Boston, yesterday morning, June 14. The Convention was called to order by Mayor Lincoln, and temporarily organized by the choice of Dr. John H. Briscoe, of Pennsylvania, as President, and Dr. Henry G. Clark, of Boston, as Secretary. Alderman Starr, of New York, moved that a committee of one be appointed from each State to nominate a list of permanent officers; and the following gentlemen were appointed on the committee:

New York, C. C. Savage; Massachusetts, Joseph M. Wightman; Pennsylvania, Dr. Ruschenberger; New Jersey, Isaac A. Nichols; Rhode Island, Dr. Snow; Maryland, J. W. Houck; Georgia, Rich'd D. Arnold; Tennessee, C. B. Guthrie; Ohio, Robert Thompson; and Dr. McLellan, delegated by the Secretary of War to attend the Convention as the representative of the medical department of the U. S. Army.

Mayor Lincoln then addressed the delegates, extending to them a cordial welcome in the name of the city of Boston. He was glad to see that every section of the country was represented, and every facility should be offered for the transaction of business, and in due time they would receive the hospitalities of the citizens.

An invitation to visit the rooms of the Natural History Society was received and accepted, together with an invitation to an excursion to-day among the institutions in the harbor, and to a banquet by the City Council, at the Revere House, on Saturday at 5 P. M.

General Wetmore, of New York, spoke of the beneficial operations of this body in regard to the quarantine restrictions in that city. The action of the Convention fourteen months ago in the matter, was second only to the Declaration of Independence.

Mr. Wightman, from the Committee on Permanent Organization, reported as follows:

President—Dr. Jacob Bigelow, of Boston. *Vice Presidents*—Hon. R. D. Arnold, of Georgia; A. H. Stevens, M. D., of New York; H. G. Clark, M. D., of Boston; John F. Lamb, M. D., of Pennsylvania; Judson Gilman, M. D., of Maryland; Hon. Moses Bigelow, of New Jersey; Hon. J. C. Knight, of Rhode Island; Robert Thompson, M. D., of Ohio; C. B. Guthrie, M. D., of Tennessee; Thomas Stewardson, M. D., of Pennsylvania; Alderman Charles Starr, of New York; Hon. Thomas Aspinwall, of Boston; J. W. Houck, M. D., of Baltimore. *Secretaries*—Calvin Ellis, M. D., of Boston; J. B. Jones, M. D., of Brooklyn; William Taylor, M. D., of Pennsylvania; Alderman David C. Dadd, jr., of New Jersey.

Upon taking the chair, Dr. Bigelow made a few remarks on the importance of Sanitary Conventions, and thanked the assembly for the honor done him.

Prayer was offered by Bishop Eastburn, after which Mr. Wightman called for reports of special committees.

Dr. C. B. Guthrie, of Tenn., presented a report upon legal restrictions for the control of the sale of poisons and dangerous drugs, ending with proposing the following form of law and list of poisons and dangerous drugs :

SEC. 1. No person shall be allowed to sell or dispense any of the following articles named in this act, and known as "Poisons, or Dangerous Drugs," except they shall hold the diploma or certificate of membership of a College of Pharmacy, or the certificate of the American Pharmaceutical Association, or the certificate of two or more physicians in regular or active practice in the town where they reside and propose to do business, certifying to their acquirements as apothecaries and integrity as men, which diploma or certificate shall be exhibited in a conspicuous place in their stores. This act shall not be considered as applying to practitioners of medicine in selling or dispensing to their patients.

SEC. 2. The following named articles shall be deemed poisons, and the persons authorized to sell or dispense the same, shall keep a book of registration, in which the name of the article and the quantity sold, and the name and sex of the purchaser shall be duly entered, and it shall not be lawful to sell or dispense the same to minors or persons of unsound mind :

List of Poisons — Atropia and its Salts; Aconite and its preparations; Arsenic; Corrosive Sublimate; Cyanide of Silver, Mercury, Zinc and Potassium; Cannabis Indica and its preparations; Cocculus Indicus and its preparations; Cantharides; Deadly Night Shade; Digitalis; Datura; Delphinia and its Salts; Ergot and its preparations; Gelseminum and its preparations; Henbane; Nux Vomica; Nicotine; Opium and its Salts; Essential Oil of Bitter Almonds, Rue, Tanzy, Savin; Prussic Acid; Picrotoxin; Poison Hemlock; Strychnine; St. Ignatius Bean; Tartar Emetic in more than six grains; Veratria.

SEC. 3. This act shall not be taken to apply to such as are engaged in the manufacture or wholesaling of any of the above articles, except when sold in small quantities to others beside the trade.

SEC. 4. All such articles shall be clearly and distinctly labelled with the name of each article, and such as are commonly used for the destruction of vermin shall also be labelled Poison before leaving the hands of the apothecary.

SEC. 5. All persons offending against the provisions of this law shall be subject to the penalty of \$—for each and every offence.

The report mentions the desirability of a law nearly uniform in all the States. The report was laid on the table.

Dr. Alexander H. Stephens of New York, presented a report from the Committee on the Utility of Wet Docks in connection with quarantines, and Dr. A. N. Bell of Brooklyn, New York, one from the Committee on external hygiene, in print, which was accepted and laid on the table, together with a letter from Dr. Reed on the same subject. A report by Dr. Snow of Providence, was also received and laid on the table.

At one o'clock the Convention adjourned till 4 P. M.

AFTERNOON SESSION.

The Convention met at four o'clock, according to adjournment, and the records of the morning's proceedings were read by the Secretary.

A communication was received from Mr. Gideon Hayes, Warden of the State Prison, inviting the Convention to visit that institution. The following gentlemen were appointed on the Business Committee: Dr. John H. Griscom, New York; Dr. John M. Moriarty, Boston; Dr. Wilson Jewell, Penn.; Dr. Judson Gilman, Ind.; Dr. John Jeffries, Mass.; Dr. Edward Meade, Ohio; Dr. E. M. Snow, Rhode Island; Joseph M. Wightman, Mass.; Dr. C. B. Guthrie, Tenn.; Dr. L. A. Sayre, New York; Dr. John F. Lamb, Penn.; Dr. Lewis W. Oakley, New Jersey; Prosper M. Wetmore, New York.

On motion of Mayor Lincoln, the several reports which were laid on the table in the morning, were referred to the Business Committee, who then retired for consultation.

Dr. Ordroneaux gave notice that he should make a motion in the morning for a committee of five to regulate what business should come before the Convention.

On motion of Hon. Moses Kimball, the Business Committee were instructed to consider the expediency of appointing a special committee to consider the subject of pleuro pneumonia. A resolve from the Business Committee, advising the legislature to take into consideration a law regulating the sale of poisonous drugs, as recommended by Dr. Guthrie, was adopted.

Dr. Jewell of Philadelphia objected to the resolution. He thought the shop boys did the mischief, and the apothecaries bore the blame. Dr. Griscom thought it very unwise for the Convention to take any steps in the matter.

Dr. Jacob Bigelow said the subject had been considered by the Convention several years ago, and they came to the conclusion, after a mature deliberation, that it was an evil to be affected by public opinion, rather than legislation. He knew of one apothecary who sold between seventy and eighty half-

ounce papers of arsenic in a year—handing it out to any one asked for it. But the people had become alarmed, and legislation was unnecessary.

Dr. Ordronaux said he should go heart and hand for the adoption of the resolution.

Dr. Guthrie explained his motives for proposing this action.

The subject was discussed at some length by Dr. Jewell of Philadelphia, Mr. Shannon of New York, and Dr. Guemsly of the same city.

Finally, on motion of Mr. Kimball, the whole subject of Dr. Guthrie's "Report upon Legal Restrictions for the control of the sale of poisonous and dangerous drugs," was indefinitely postponed, and the Convention adjourned.

4. *The Convention of Superintendents of Hospitals for the Insane.*

This Convention was held last week in Philadelphia, and was largely attended. The meetings were held in one of the rooms of the Continental Hotel. Many interesting papers were read and discussed. More than thirty Superintendents of Insane Hospitals were present during the session.

During their stay, the Association was invited to visit the different hospitals in and around the city, as well as other public institutions, but were unable to accept all the invitations received.

On Tuesday they visited the admirable institution under the charge of Dr. Kirkbride, known as the Pennsylvania Hospital for the Insane, where, after examining the building and appurtenances, they dined and held a session, spending the evening at the Doctor's house.

In the afternoon they visited the Friends' Asylum for the Insane at Frankford, and after viewing the building and grounds, they partook of an elegant collation prepared by Dr. Worthington.

On Friday they visited the department for the Insane at the Philadelphia Hospital, Blockley.

EDITORIAL AND MISCELLANEOUS.

VOLUME XV.

In commencing a new volume of the Maryland and Virginia Medical Journal, our greatest hope is to make the second better than the first. Our facilities are increasing. Our foreign exchanges are becoming more numerous. Physicians everywhere seem pleased with what has already been done. The subscription list is growing larger and larger. But few refuse the Journal, and all are anxious both to encourage and help on the work. We made certain promises in January with the first issue. Let us enumerate some of them, and ask how they have been fulfilled.

1st. We stated that arrangements had been made by which we hoped to offer "an appreciative review of all the medical works issued from the leading publishing houses in this country." The books that have been reviewed have been studied with care; and all those received by us for review would have been equally carefully noticed but for the sickness and death of the gentleman having the labor in charge. We have physicians of cultivated taste, enlarged reading, and discriminating judgment, who have themselves felt the necessity of this branch of periodical literature so forcibly, that they are anxious to embrace the opportunity now offered to assist their medical brethren, and therefore cheerfully consent to continue to labor in this department with us.

2dly. We promised "a selected periscope of foreign practice," which we have sedulously labored to fulfill. But as we before stated, the foreign exchanges are increasing, and with the assistance of active translators, we shall be greatly disappointed if this department is not improved with each and every volume.

And 3dly. We promised "the result of the experience of our most zealous physicians." We refer to the volume just issued

with pleasure for the redeeming of the promise given by us to the public. The original matter contained in the volume just completed will probably, on review, strike us all with surprise; and we find it is fully appreciated and accredited wherever the Journal has gone. We said, in January, the intrinsic merits of the Journal must depend upon the profession at large, within the limits of its circulation, and upon those who feel an interest in its welfare. It is necessary that every enterprise should have a central point and a nominal head. But such is the catholicity of science, that it is hoped each medical gentleman will consider it as much his organ for the dissemination of all useful information relative to our common profession, as if he himself appeared as editor. It is indeed the editor's duty only to separate the pseudo from the real, as far as possible, and to give each true medical philosopher a voice among his brethren. We can now speak from experience that the undertaking of being even the "nominal head" is not without its cares, its responsibilities, and its pleasures. It is a care and responsibility to know that the eyes of the profession are upon us, associated as we are with the success or failure of the undertaking. It is a pleasure to find that these labors ("undertaken for the sake of the work itself") are appreciated by many. There are others incredulous to the fact stated, and to these we can only appeal by its reiteration.

In order to form some conception of the value of a journal we have only to imagine a profession at the present day existing *without* one. We hope we are as sensibly alive to the excesses of journalism as any one can be, who holds a high respect for exact information. And this is the very principle from which the Maryland and Virginia Medical Journal would be glad to owe its origin. The more frequent publications—the larger quarterlies, the semi-annual and annual abstracts of medical science—are examples of the necessity of further revision and more careful selection of medical matters. It is not our intention by this remark to draw a comparison between this journal and any one of these publications; but it is our wish to draw attention to the labor taken to select the most

important matters for publication, and this without prejudice or interested personal motives. In this labor we invite all who have the leisure and inclination to join us. The interest and the improvement of the profession is the sole object in view.

It is no longer a disputed point that the localization of a journal has a beneficial influence over a circumscribed locality. When properly appreciated at home, the further it recedes the more diluted should be its influence upon certain points of practice. There are general principles of medicine recognized as important everywhere; but there are also certain local treatments of disease, the true value of which is only obtained by experience. Our direction is plain, being the central point of the two States. The publication of our motives is the strongest guarantee of sincerity we can give. We labor only as a common member of a profession for its general advantage and success, to which aim we invite all its members.

THE ASSOCIATION AND THE COLLEGES.

Our columns are crowded with the proceedings of the late reunion of the medical profession, and we have barely space to notice very briefly some of the incidents of that occasion.

The representatives of *twenty-one* medical colleges meeting in convention, simultaneously with the Association, composed of more than *five hundred* delegates from the American profession, amidst the profuse hospitalities and picturesque groves of New Haven, gave an opportunity for an interchange of views between different sections and interests, which ought naturally to have been productive of good, and we have reason to hope that such a desirable consummation may result from the consultations and deliberations of the late meeting.

On the important subject of *medical education*, the schools and the Association approximated more closely than on any other occasion, as will be seen by an examination of the reported proceedings. Their joint resolutions contain the following leading features:

1st. A more careful scrutiny as to the previous course of study pursued by the medical student.

2d. A rigid demand of the student that he should give some evidence of having studied disease at the bedside.

3d. A refusal to permit students to attend two courses of lectures without having an intermission of "*at least three months*" between each course, or as amended by the Association, that no student should be regarded as eligible for graduation who begins his second course of lectures in less than *one year* after he commenced his first.

On these three points the Association and the Colleges acceded with great unanimity. The fourth important suggestion made by the profession to the colleges was a proposition to admit the representatives of State societies to the examinations of students, so that unworthy applicants for the degree might be excluded from the profession.

The Convention of the Colleges hesitated to accept this radical alteration of their customs, and this is not to be wondered at, when we remember that the four largest schools in the Union were not represented, and of course would not consider themselves bound by such resolutions. But the Association insisted on the propriety of this new regulation, and as an encouragement to the schools, acceded to a proposition to append its *seal of approbation* to the diplomas of every institution which will heartily and earnestly come up to their standard of requirement.

It will be observed that the schools and the profession stand for the first time upon a common platform, the only point of difference being upon the propriety of admitting the presence of the laity into the examination room, an innovation of established rules, which, under the circumstances, very naturally demanded a cautious and well-considered policy.

We will say, however, that many of the teachers present were prepared to throw open their colleges to the profession during examinations, not fearing to submit their candidates to the scrutiny of their medical brethren, and they hesitated only to declare their willingness to adopt such a plan, because they required time to consult with their colleagues, and mainly be-

cause of the absence of the two leading Philadelphia schools, the University of New York and the University of Nashville, whose co-operation in such a step was certainly to be desired.

In another number we intend to give our views in full upon this important question, and we conclude for the present this hasty notice of the subject.

TRIBUTE OF RESPECT.

At a meeting of the Petersburg Medical Faculty, held at the office of Dr. W. J. Michie, President, Monday, June 18th, at 4 o'clock P. M., for the purpose of paying a tribute of respect to the memory of Dr. LUKE WHITE, deceased, late a member of the Faculty,

The President having the chair, the meeting was called to order, and on a motion, a committee was appointed to express the sense of the Medical Faculty in relation to the affecting event. Said committee, consisting of Doctors, Jas. May, J. J. Thweatt, J. H. Claiborne, J. W. Smith and C. F. Couch, after the necessary deliberation, reported the following preamble and resolutions, which were unanimously adopted:

WHEREAS, it has pleased an all-wise God to visit us and this community with a most afflicting dispensation, in the death of Dr. Luke White, a venerable member and friend, who on the morning of the 18th, full of honors and of years, was gathered to his fathers. In memory of his many virtues as a man, and of his exalted character as a physician, the Petersburg Medical Faculty do unanimously adopt the following resolutions as testimonials of their respect and veneration for the deceased.

Resolved, that we deeply sympathize with the bereaved family of the deceased, and with this community, who have sustained an irreparable loss in the death of Dr. Luke White.

Resolved, That as a further testimonial of our respect for the memory of the deceased, we will attend his funeral in a body, and wear the usual badge of mourning for thirty days.

Resolved, That the Secretary be requested to transmit a copy of these resolutions to the family of the deceased, and cause them to be published in the Virginia Medical Journal, and in the newspapers of the city.

W. J. MICHIE, M. D.,
President Petersburg Medical Faculty.

C. F. COUCH, M. D.,
Secretary Petersburg Medical Faculty.

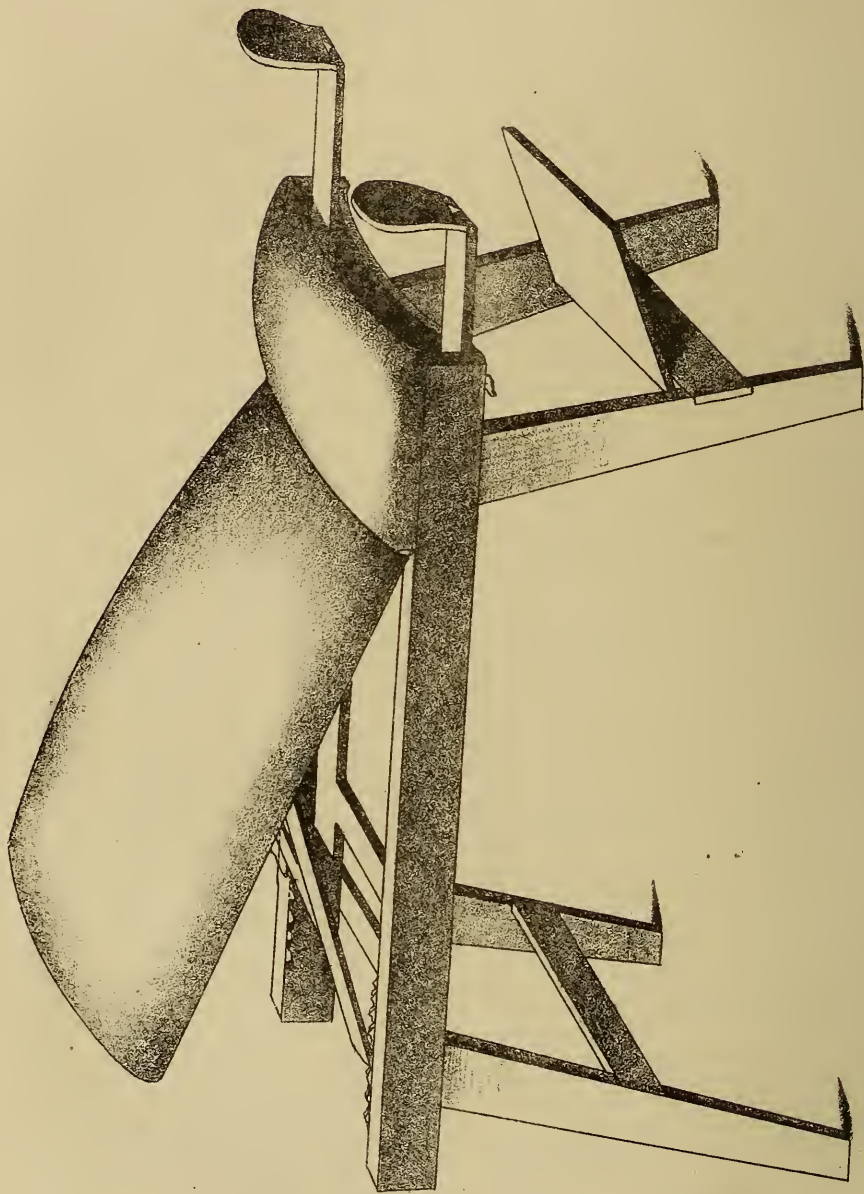
VARIETIES.

ABDUL-MEDJIDH, though a very moderate ruler, has an excellent idea of the value of physic. In a late tour through his provinces he picked up a fever, and very happily so for his doctor, Caratheodory. A few doses of sulphate of quinine appear to have put the Imperial patient on his legs again, and so relieved the tremblings of an anxious list of court hangers-on. Now, it appears, that when the Sultan is proclaimed convalescent after an illness, it is the custom of all the members of the Imperial family, for all the high dignitaries and functionaries, to make the presiding Medical genius a present. Of course the degree of these gentlemen's joy is measured by the size of their present; and the joy being very great on the occasion in question, Dr. Caratheodory has come in for some three or four hundred thousand francs' worth of jewels, objects of art, and presents of all kinds. Then comes the Sultan's turn, and for his fee he gives the Doctor a magnificent domain. It would seem to be only among demi-civilized and savage nations that the Doctor receives proper respect. However, there may be a reverse to the medal even here; for we are not told what M. Caratheodory's fee would have been if his affectionate friends had lost their beloved master Abdul! All the world cried out against M. Lallemand when he charged Ibrahim Pacha 200,000 francs for an operation on the urethra; but it is clear that Lallemand understood the ways of Orientals.

THE following extract from Mr. Bertherand's interesting letters on the Italian campaign, points out the use of adulterated spirits as a fruitful cause of illness in the French army:

"The art of adulterating wines," says Dr. Bertherand, "was doubtless invented by sutlers, desirous of evading the cost and risks of carriage.

"The following is the recipe for the fabrication of cheap but unwholesome wine: Water from a neighboring ditch, alum, beet root juice, corn spirit; of each a quant. suff. according to the liquid and vintage in demand, mixed up in an old cask." It is difficult to imagine a more expeditious or economical way of preparing *rosy wine*.



Johnstone's operating Chair.

Illustration of Hoyer & Ludvig, Richmond.

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New Series.

ART. I.—*Clinical Lecture on Diabetes Mellitus*; delivered in the Baltimore Infirmary, by SAMUEL CHEW, M. D., Professor of Principles and Practice of Medicine in the University of Maryland.

GENTLEMEN: I request your attention this morning to the subject of Diabetes Mellitus, a remarkable disease, of which we have recently had two cases in the wards of this hospital. During the time that these cases were under treatment, you had daily and ample opportunity for examining their symptoms; all of you, I believe, availed yourselves of this opportunity with becoming zeal and diligence; and now, that the patients have left the house, it may, perhaps, assist you in your studies to have the observations which were made while they were under our care recalled to your memory, and collated with the accounts of the disease which you find in your books.

In the first place, let me lay before you the histories of the two cases as they were drawn up by our accurate Clinical Clerk, Dr. Clinton Wagner.

CASE I.—John K——, a native of the State of Georgia, twenty-five years of age, by occupation a sailor, was admitted January 19, 1860. He stated that he had suffered from several severe attacks of malarious fever, the last of which occurred somewhat more than three years ago, and was followed

by ascites and œdema of the lower extremities. Before the dropsy had entirely disappeared, he observed an enormous increase in his daily excretion of urine, accompanied by inordinate thirst, dryness of the skin, constipation of the bowels, emaciation, and loss of strength. His appetite for food was unimpaired. These symptoms had continued up to the time of his entering the Infirmary, and, in addition to them, he complained of pain and tenderness in the abdominal and lumbar regions, of great restlessness at night, and of redness and burning about the orifice of the urethra. The amount of urine which he has voided daily during the last three years has ranged, he says, from twenty-five to thirty pints. While he was in the Infirmary, the daily quantity varied from fifteen to twenty-two pints, with a specific gravity of from 1.030 to 1.040. On applying the usual tests, there was evidence of the presence in the fluid of a large proportion of sugar. He was ordered to be confined to a diet consisting exclusively of animal food. To this restriction it was found that he could never submit more than three or four days in succession without suffering from severe symptoms of dysentery, which were relieved upon his resorting to the use of vegetable aliment. Dover's powder, carbonate of ammonia and rennet were, at different times, prescribed for him. Each of these agents appeared to exert some degree of beneficial influence over his malady; and while he was limited to animal diet, the quantity of his urine was considerably lessened. His temper, however, was irritable, gloomy and fickle; he was unwilling to submit to medical treatment; he thought he could do better in the country; and finally left the house on the 6th of February.

CASE II.—Jacob R——, a colored agricultural laborer, native of Maryland, aged forty-eight years, entered the house April 28, 1860.

He stated that four years ago, after exposure to wet and cold weather, he was attacked by a chill, followed by a high fever. Shortly after this, he first noticed a very great increase in the quantity of his urine, which has continued up to the present time. During the last four years his strength and weight

have been greatly diminished; his appetite has been good; his thirst constant and distressing; his skin hardly ever moistened by the least perspiration. He suffers from great restlessness at night, and from continued pain in the lumbar region. His urine was found to contain a large proportion of sugar. The following is an accurate record of the daily quantity and specific gravity of the secretion during the first twelve days which the patient spent in the Infirmary:

April 28.—	160 ounces;	specific gravity	. 1.030
29.—	140 “	“	. 1.028
30.—	110 “	“	. 1.024
May 1.—	160 “	“	. 1.024
2.—	200 “	“	. 1.024
3 —	128 “	“	. 1.026
4.—	96 “	“	. 1.026
5.—	128 “	“	. 1.026
6.—	90 “	“	. 1.026
7.—	88 “	“	. 1.026
8.—	120 “	“	. 1.027
9.—	160 “	“	. 1.033

The treatment consisted of confinement to animal diet, and the use of Dover's powder and warm baths. He was benefited in some measure by these means; the quantity of the urine was reduced, and it contained a smaller proportion of sugar; but the patient was compelled to leave the house on the 11th of May, before there had been time for any material or permanent improvement.

Each of these patients had, before he came into the Infirmary, accidentally observed the extremely sweet taste of his urine.

Such were the cases—their symptoms and their histories. Now, the first thing to be done was, of course, to ascertain, from what we had heard and observed, the correct *diagnosis* of the disease. In relation to this question there was no difficulty. The great increase in the quantity of the urinary secretion, and the long persistency of that functional disorder, at once excited suspicion that the disease was Diabetes Mel-

litus. The detection of sugar in the urine proved that this suspicion was correct.

You will not, in every instance, find the diagnosis of diabetes so easy and evident. It is, occasionally, extremely difficult. You may fail to detect the disease at its beginning, and you may equally fail at its ending. At its commencement, diabetes generally escapes detection. In its early stage, when there is but little sugar in the urine, the quantity of that secretion is scarcely at all—or not at all—increased, and there may be nothing to direct attention to the true nature of the case. And, again, if you chance to see the patient for the first time only a few days before his death, you will sometimes find his urine wholly destitute of sugar; that substance in some instances ceases to occur in the urine a short time before the fatal termination; and, in that case, if you are unacquainted with the previous symptoms, you will necessarily be very much at a loss to form a correct opinion respecting the disease.

Diuresis to be distinguished from Diabetes.—It is important that you should be on your guard against the error, which is sometimes committed, of mistaking every case of large and persistent increase of the urinary secretion for diabetes mellitus. A temporary increase of the urine is of very frequent occurrence. It often happens, independently of disease, in consequence of the action of cold and damp air on the surface of the body. It is often observed as a symptom of certain diseases, as asthma, hysteria, and some other nervous affections. In such instances it is generally of very brief duration. But it also occurs as a permanent and chronic disorder, and, in that case, bears a very strong resemblance to diabetes. It has frequently been mistaken for that disease, but is distinguished from it by the important circumstance that there is no sugar in the urine. You all remember a case of this kind—Diabetes Insipidus, as it is called by some writers, Chronic Diuresis as it may more properly be termed,—which I had an opportunity of showing you a month or two ago. The patient, a man of the name of Barker, entered the Infirmary in March

last, suffering excessively from an unnaturally large flow of urine. The disorder had commenced four months previously, and, during all that time, the daily discharge of water had, according to his representation, been enormous—such as might have astonished Captain Gulliver, even after his experience among the Maids of Honor in Brobdingnag. When he came under our care, he was feeble and emaciated, his thirst was extreme, his skin dry, his bowels constipated, and he voided every day about twenty pints of urine. We all thought that his malady was probably diabetes mellitus. But, on careful examination, his urine was found to present not the least trace of sugar, and its specific gravity, so far from being increased, was considerably less than natural, being only from 1.002 to 1.004. This man was speedily relieved and restored to health, a result entirely the reverse, as you are aware, of what we have to expect in cases of true diabetes.

Symptoms. The two cases of diabetes which you have witnessed afforded you a pretty good manifestation of the ordinary phenomena of the disease. In neither of them was the patient able to give an accurate or satisfactory account of his early symptoms. And this is what you will generally observe. The onset of the disorder is almost always gradual, mild and insidious. Its incipient symptoms are trivial or equivocal; they are commonly overlooked and neglected; and it is difficult, or in most instances impossible, to ascertain with precision at what time the disease commenced. The symptom which first attracts attention is increase in the quantity of the urine. The patient finds it necessary to empty his bladder more frequently than usual, and is compelled to rise for that purpose several times every night. The urine is unnaturally clear and pale; its taste is in many cases observed to be sweet; and its customary odor is diminished or wholly lost.

This disorder of the urinary function was the prominent symptom in both of the cases which we are considering. In both of them it was accompanied by evidences of derangement of the organs of nutrition. Both of the patients were extremely emaciated, though they consumed habitually an abund-

ance of food. Both of them suffered from constipation, and from tenderness and pains in the abdomen.

You will generally observe that the mouth through the whole course of the disease is clammy, and the tongue unnatural in its appearance; in some cases loaded with white mucus; in some, red over its whole surface; or red on its edges and with a streak of brown fur along its middle. The gums are red and irritable; the breath of a peculiar sweetish odor; and the throat dry.

The patient suffers from constant thirst. He will often tell you that this is the worst part of his distress. Occasionally dyspeptic symptoms occur; but in general the appetite is not impaired; on the contrary, it is often stronger than natural. Food is taken eagerly and in large quantities; yet the bulk and vigor of the system are constantly decreasing.

The skin is almost always dry and harsh; so much fluid is lost from the urinary organs, that there is little or none left to escape by the surface, or by the other emunctories.

The pulse is in general soft and feeble, natural in frequency, but easily excited to increased rapidity. The blood, during the early stage of the disease, presents, in some cases, a buffy coating. Its serum is whey-like in appearance, owing probably to the presence of fatty matter thrown into it by the absorbents. But the most remarkable and most characteristic change in the circulating fluid is that it contains sugar, which, by an accurate analysis, may be detected in the contents both of the arteries and veins.

One of our two patients suffered from inflammation of the external orifice of the urethra. This is an ordinary symptom of the disease, and is probably occasioned by the unnatural and irritating quality of the urine; a circumstance which also accounts for the phymosis which not infrequently occurs. The anaphrodisia which has been noticed as belonging to diabetes is always experienced sooner or later in the progress of the disorder, but probably not in a greater degree than in any other malady that produces equal debility and exhaustion.

As the disease advances, the patient complains of chilliness,

and becomes extremely sensitive to cold. His feet are alternately cold and burning, and the ankles are, in many cases, affected by slight œdema.

The mind is, after a time, more or less involved; it suffers with the suffering body; it is disordered, both in its intellectual and moral faculties; the power of attention is diminished; the will is vacillating and fickle, and the temper is selfish, irritable, fretful, anxious, melancholy and despondent. Diabetic patients are, in general, extremely troublesome, and you will rarely derive any pleasure or credit from attending them. One of our two was perverse, unreasonable, wrong-headed, and of very ill conditions. The other, the colored man, was apparently not deficient in gentleness and amiability, and in that "sufferance which is the badge of all his tribe"; but he was uniformly dejected and hopeless, and had evidently but small faith in our power to relieve him. The change in the spirits and temper, caused by the disease, you will not be surprised at, nor inclined to censure, when you consider the nature of the patient's case. There is certainly nothing very cheering or exhilarating in an attack of diabetes; and we know, too, that the members of our profession, in dealing with the subjects of this disease, not infrequently present a resemblance, more close than pleasing, to those eastern sages who, with such unhappy success, took upon themselves the task of consoling the Patriarch of Uz.

Course and Terminations.—With the symptoms that have been stated, the disease often persists for many months, not very rarely for several years, undergoing but little apparent change, except that the weakness and emaciation are gradually or rapidly increasing. Death at length takes place; in some cases from exhaustion, in some from sudden sinking, in others, and most commonly, from the supervention of some secondary malady. A very large majority of cases terminate by phthisis; some by apoplexy; some by albuminuria; some by dropsy; some by peritonitis. Our cases had run on, the one for four, the other for more than three years; and neither of them appeared to be in any immediate danger of a fatal termination.

Changes in the Urine.—Of all the phenomena of diabetes the most important and remarkable are the changes in the character of the urine.

The urine is very generally increased in quantity and in specific gravity; its sensible qualities and its chemical composition are conspicuously altered; and it contains in all cases an appreciable amount of sugar, a substance never found in the healthy secretion.

Quantity.—The urine of a healthy adult is from two to four pints in every twenty-four hours. Its average is probably about two pints and a half, or forty ounces. In diabetes the quantity is almost always greatly increased; not always; for the disease has been known to exist for a considerable time, and the urine to be extremely saccharine, with little or no augmentation of its quantity. Such instances, however, are exceptional and rare. Very generally the quantity is unnaturally abundant. The patient, instead of the usual two pints and a half, voids sometimes as much as forty pints in twenty-four hours. Seventy pints—and in one extremely apocryphal case over two hundred pints—are said to have passed in the same period. From twenty-five to thirty-five pints have in many cases been discharged daily for weeks, and even for months, in succession. Thirteen and fourteen pints daily are by no means uncommon. The quantity is seldom under ten pints; and the average is probably between fifteen and twenty-five pints. It is rarely as low as the natural standard except when a favorable impression has been made on the disease by medical treatment.

The quantity of the urine often exceeds habitually that of all the fluids which the patient takes both in his food and drink. It is sometimes twice as large, and in a case treated by Dr. Christison it was for several days five times as large as the aggregate of the liquids in all the ingesta. This enormous increase is supplied in part by the absorption of the fluids of the body, and in a still greater degree by the entrance into the system of the watery vapor of the atmosphere through the skin and bronchial membrane.

Specific Gravity.—The specific gravity of diabetic urine is usually very considerably increased; and this is the more remarkable as it occurs at the same time with increase in the quantity of the secretion.

The sp. grav. of healthy urine varies from 1.015 to 1.025. That of diabetic urine ranges from 1.020 to 1.050. Dr. Prout had in one case found it at 1.010; but it is rarely as low as 1.020, and it is sometimes as high as 1.060. It may be very little above the healthy standard when sugar is present in only small quantity. Its average is said to be 1.040.

Whenever the urine is abundant, and its sp. grav. as high as 1.030 or near that point, diabetes should be suspected.

Solid Contents.—The increased specific gravity depends upon an increase in the quantity of solids contained in the urine. In healthy urine, the proportion of solids varies from 30 to 68 parts in the thousand. In the urine of diabetes it rises to 90, 100, 120, and even to 136 in the thousand. The absolute quantity in health rarely exceeds $2\frac{1}{2}$ ounces daily. In diabetes it is often 33 ounces daily, and has been known to be $36\frac{1}{2}$ ounces daily for weeks in succession. The solids discharged daily in the urine are often much larger in quantity than those received into the system in the same time in the form of food.

Sensible Qualities.—In the sensible qualities and chemical composition of the urine various changes are observed. The fluid is paler than natural—of a pale straw or greenish-yellow color. It throws down little or no deposit on cooling. Its odor is peculiar and has been compared by different observers to that of hay, of whey, of milk, of violets, of apples, of a *whale*, a *weasel*, a *camel*, and of many other things; it has certainly little or no resemblance to the odor of healthy urine. The *urea* was long supposed to be deficient in quantity, and was thought to be in some manner converted into sugar. It is now, however, known to be not less, and in some cases more, abundant than natural. Lithic acid and the usual saline ingredients of healthy urine are present, and in their just relative proportion, but of course very much diluted by the great increase of the watery part of the secretion.

Sugar.—But the most peculiar and distinctive of all the changes in the urine is the occurrence of sugar in that secretion. This substance is often perceptible to the taste, and is commonly discovered in this manner by the patient. The absence of a sweet taste does not, however, prove that the fluid is destitute of sugar; for not infrequently the urine is not sweet in cases where one-half of its solid ingredients consists of saccharine matter, the sweetness being concealed or obscured by the strong taste of the other constituents. We are compelled, therefore, to resort to more delicate tests than the sense of taste. The tests commonly depended on are Moore's, Trommer's, and the production of fermentation. With all of these I know you are familiar, as you have repeatedly seen them employed, and often employed them yourselves. Yet as the subject is important, you will allow me to refresh your memory by repeating to you the directions for their use.

Moore's Test consists in adding to a small quantity of the suspected urine about half its volume of liquor potassæ, and boiling the mixture gently for five minutes, when if sugar is present the fluid assumes a brownish or bistre hue. Little or no increase of color takes place when the urine is destitute of sugar.

This is a very convenient test; but in some rare cases it may lead to an erroneous conclusion. It is advisable, therefore, when there is the least ground for doubt, to resort to the other modes of examination.

Trommer's Test depends on the fact that when a fluid containing diabetic sugar is boiled with a mixture of potash and sulphate of copper, the oxide of copper in the sulphate is reduced to a suboxide, which is precipitated in the form of a reddish-brown granular powder.

In applying this test, you add to a small quantity of the suspected urine as much of a solution of sulphate of copper as is sufficient to communicate a very pale blue tint. You then add liquor potassæ in quantity equal to about half the volume of urine employed. This throws down a precipitate of hydrated oxide of copper, which, if sugar is present, is immediately

re-dissolved, forming a purplish-blue solution. Upon gently boiling this for a few minutes, if sugar is present, a reddish or yellowish-brown precipitate of sub-oxide of copper is deposited from the liquid.

The Test by Fermentation is extremely delicate and accurate. It is capable of detecting half a grain of sugar in two ounces of liquid. You apply it by filling a tube with the suspected urine, to which a few drops of fresh yeast have been added. The tube is inverted into a saucer containing more of the same urine, care being used to prevent the entrance of air. It is then placed in a temperature of 70° or 80°. If sugar is present, the fluid begins almost immediately to undergo the vinous fermentation. Carbonic-acid gas is formed, rises in small bubbles, and collecting in the upper part of the tube displaces the liquid, which escapes through the open end into the saucer. When the quantity of sugar is at all considerable, the urine after fermentation presents a faint vinous smell, owing to the alcohol formed during the process. There is so much alcohol in some cases that the liquid blazes when thrown into the fire.

Character of Diabetic Sugar.—The sugar found in diabetic urine differs in some respects from common sugar, the product of the sugar cane. It is much less sweet to the taste, and it is harder and less soluble in water. It is more closely similar to the sugar contained in grapes and in most other kinds of fruit, and which is known by the name of *glucose*. In quantity, it varies from the slightest trace to 50 or 80 parts in the thousand. Its absolute amount is in some instances enormous. In several cases examined by Dr. Garrod, it was found that from half a pound to a pound and three-quarters was excreted daily. It may be obtained in a solid form by evaporating the urine in a steam bath till it is reduced to one-half, pouring this into a shallow dish and setting it aside for a few days, when the sugar is gradually deposited in an irregularly crystalline mass. Some of you, I believe, prepared samples of very commendable quality from the urine of the colored patient, and suggested to him that if he would emigrate to Louisiana he might perhaps find employment as an especially valuable hand on a sugar plantation.

Pathology.—Respecting the essential nature of diabetics, we possess but very little satisfactory knowledge. Anatomy affords us only negative information. There is no lesion, no change of structure, as yet discovered, which occurs in all cases, or even in a majority of cases, and none which in any case is sufficient to account for the existence of the disease.

The difficulty is to explain the extraordinary formation of sugar in the system. This substance has been detected in both the arterial and venous blood of diabetic patients; and when sugar exists in the blood, we can readily understand that it may be separated from that fluid by the kidneys, and eliminated from the system along with the urine. We can understand also that in passing through the kidneys it may excite those organs to such increased action as to occasion profuse diuresis, and thus give rise to the thirst, the hunger, the emaciation, the debility, and many of the other symptoms which ordinarily attend the disease. But the question still remains, where and why,—in what organ and from what cause,—is the sugar formed which we find in diabetic blood; and to this inquiry no satisfactory reply has as yet been given.

The primary disease upon which the formation of sugar depends has at different times, and by different speculators, been referred to various organs—to the kidneys, the stomach, the liver, the lungs, the brain. No one knows where it is situated, or what is its nature.

With regard to the presence of sugar in the system before it reaches the urine, several interesting facts have been ascertained.

It was discovered by Mr. Macgregor, of Glasgow, that the chyme formed in the stomach of persons laboring under diabetes, always contains sugar, even when the patients have for several days been debarred from the use of vegetable food, or of any aliment that is known to be capable of conversion into sugar. The chyme produced under the same circumstances in the stomach of healthy persons was found to be wholly destitute of saccharine matter. The occurrence of sugar in the chyme, and its subsequent appearance in the blood and in the urine

were supposed to be occasioned by some defect in the digestive function; and this defect was believed to be the essential disease upon which diabetes depends. But the nature and cause of this disease neither Mr. Macgregor nor any one else has explained.

The researches of M. Claude Bernard have shown that sugar is formed in the system independently of any diseased condition; that its formation is one of the regular processes of the healthy economy, and occurs in the bodies not only of human beings, but of all other animals. M. Bernard discovered that the tissues of the liver differ from those of all other parts of the body, in the remarkable circumstance of always containing sugar; that this substance does not exist in the blood of the Portal vein; but that it may always be found in that of the Hepatic veins, even when the food has for months previously been exclusively of an animal nature. The sugar in the hepatic veins passes along those vessels into the ascending cava, into the right auricle and ventricle, and into the pulmonary artery. In all of these situations it may be detected. In the lungs it disappears. It enters those organs with the blood of the pulmonary artery; but the whole of it, or the greater part of it, is consumed or lost in the process of respiration, being converted into some other substance—as some think into lactic acid. Some portion of the sugar—probably only a very small portion—is said, however, to escape from the lungs and to reach the left chambers of the heart, whence it passes into the general circulation, and finally disappears in the capillaries of the aortic system.

The detection of sugar in the liver is not the only discovery of M. Bernard in relation to diabetes. He has further ascertained that this disease may be artificially produced in the lower animals by irritating—by a wound or a slight galvanic shock—that part of the floor of the fourth ventricle near which the pneumogastric nerves take their origin. In a few hours after the infliction of such an injury, the urine of the animal begins to afford traces of sugar, and it continues saccharine until the lesion of the brain has been repaired.

From these observations of M. Bernard, it has been inferred that diabetes may depend upon the fact of sugar being, in con-

sequence of diseased action of the liver, formed in that organ in excessive quantity—in quantity too large to be consumed in the lungs—or upon failure, from some cause, on the part of the lungs themselves to accomplish in the ordinary and proper quantity of sugar sent into them from the liver those changes which naturally occur during the healthy performance of the process of respiration. In other words, it has been concluded that diabetes arises from disorder either of the liver or of the lungs—either from too large a quantity of sugar being formed in the liver, or too small a quantity being consumed in the lungs; in either case the excess of sugar which has passed through the lungs entering the general circulation and acting as a source of irritation to the kidneys.

It is a circumstance which appears somewhat to conflict with this view, that the sugar formed in the liver in health, or produced in the system by irritating the brain, is not identical with the sugar of ordinary diabetes. It differs from diabetic sugar in certain of its chemical qualities, and also in the peculiarity of being much more easily decomposed when kept in contact with blood or other animal substances.

If we think fit to waive this objection as minute and unimportant, and admit that diabetes may arise in all cases from disease of the liver, of the lungs, or of the brain, we are still met by the difficulty before referred to, that with this primary disease we have no acquaintance—that we know little or nothing of its nature or of its causes.

The researches of pathologists have thus far failed to explain the origin and proximate cause of diabetes. In the progress of science, further discoveries may perhaps be made, which, added to what is already known, may serve as the materials of a true and satisfactory theory of the disease. In the meantime, it is well for us to be aware of the deficiencies of our present knowledge.

External Causes.—As diabetes occurs in most cases gradually and imperceptibly, and has generally been running on for some time before it is detected, its remote causes can seldom be discovered. Respecting them the patients can rarely give

any information. Neither of those whom you have seen could tell what occasioned his disease. In one of them it was preceded by malarious fever and dropsy; in the other, by exposure to cold and wet weather; but what influence those circumstances had exerted in producing the disease it would be difficult to pronounce. Diabetes occurs to persons accustomed to very different and opposite modes of life. In some few instances it has appeared to be excited by sudden changes of atmospheric temperature; in some, by the drinking of cold water when the body was heated and exhausted; in some, by habitual intoxication and consequent frequent physical exposures; in some, by attacks of rheumatism and of gout; and in many by anxiety and distress of mind. But it has also frequently fallen upon the prudent, the sober, the cheerful, and those who have lived in the midst of ease and comfort. It has been often observed to accompany malignant boils and carbuncles. Not infrequently it has manifested an hereditary or family tendency, several members of the same family being its victims. It is more common in the male than in the female sex, and seldom happens to infants or to old persons. Its occurrence is said to be favored by damp and cold situations; by poor and unwholesome diet; by the too free use of sugar; by venereal excesses; and by any cause which tends to disorder and enfeeble the function of digestion and assimilation.

Frequency.—Though not one of the rarest forms of disease, diabetes is by no means of common or frequent occurrence. Dr. Heberden, who had long practiced his profession very extensively in London, tells us that he had seen twenty cases which were, or were supposed to be, diabetes; but some of them, he says, seemed not to deserve the name. Dr. Christison states that, in twenty-one years, he had had occasion to know the particulars of at least sixty cases treated in the Edinburgh Infirmary by himself or his colleagues. Dr. Prout, in twenty-five years, had seen more or less of upwards of five hundred cases. He estimates that in England and Wales, on an average, one person annually in every 77,000 dies of diabetes; the proportion of fatal cases being in males to females

as 5 to 2. The mortality, he says, is in some places greatly above the average. It is greatest in Manchester, where one person annually in 16,000 dies of this affection. In Liverpool, the proportion of fatal cases is nine times less, being only one in 136,000. In Baltimore, according to the Annual Report of the Board of Health, there was in the year 1859 only one death by diabetes among a mortality from all diseases of 4,674. From 1850 to 1859 inclusive, the whole number of fatal cases of diabetes in this city was twelve—but little more than one a year from a population of more than 250,000. It is probable, however, that the real number of deaths by this malady is larger than would thus appear. The immediate cause of the fatal termination in most instances of diabetes is phthisis; and the result is then generally returned as having arisen from the pulmonary affection, and without any reference to the preceding disorder.

Prognosis.—The prognosis of diabetes is, as every one knows, exceedingly unfavorable. The result of its treatment, which you have witnessed, is not worse than that generally attained by other physicians. A perfect cure is rarely, if ever, accomplished. The urine is hardly ever restored to a permanently natural condition as to quantity and composition. However improved it may be, you will very seldom find it presenting a perfectly normal character. Its diabetic peculiarities are very apt to return, and, sooner or later, the disease is in almost all cases fully re-established, in spite of the best treatment that has as yet been devised.

Dr. Christison, among the sixty cases which he had witnessed, had seen some that were very much improved and relieved, but not one that was cured, or that terminated otherwise than fatally.

But the physician should always encourage hope, not only in his patient, but also in his own mind. It is never too late to despond. It cannot be pronounced that diabetes is necessarily and in all instances incurable. Dr. Rollo has published a case in which the patient appears to have recovered; a few similar instances have been reported by other observers; and

Dr. Bardsley asserts, that of twenty-nine which had been under his care, as many as eight had resulted in perfect recovery.

The general opinion of those who have had opportunities for observation and experiment appears to be, that the disease may be mitigated, the patient's sufferings relieved, and his life prolonged; but that a cure cannot be effected. By timely and judicious treatment, the fatal termination may often be deferred for several years—sometimes for eight or ten years.

Diabetes sometimes commences abruptly in an acute form, with active febrile disturbance, and runs on rapidly to the destruction of life. Far more commonly it is of chronic character.

The prospect of benefit from treatment is best when the symptoms are moderate in degree; when the case receives early attention; when, with proper regulation of diet and regimen, the quantity of urine is not above twelve pints daily, and its density not more than 1.035; when there is not much dryness of the skin, or wasting of the general system; when the thirst and appetite are not excessive; when there is no structural disease of the lungs or of any other organ; and when the mind is not much disordered or depressed.

Incidental inflammatory diseases are extremely apt to be intractable and fatal when they attack persons suffering from diabetes.

The occurrence of albumen in the urine, or of tuberculous disease in the lungs, or of sudden and extreme prostration, is in general speedily followed by death.

Treatment.—The most remarkable feats of our profession in connection with diabetes have been rather to detect with accuracy its existence, and to produce it artificially under instruction of M. Bernard, than to subdue and remove it by therapeutic measures.

I hope it will not occur to the enemies of our venerable fraternity to compare us to those Egyptian sorcerers who were unable to deliver their country from the plagues that afflicted it, but, by dint of their art, succeeded in bringing on it a large addition of blood and frogs.

Various methods of treating the disease have been recommended; some of them very ingenious, and which ought to succeed, but do not; like Galba, *omnium consensu capax imperii, nisi imperâsset*.

The means employed in the cases you have seen, and which I have generally found more useful than any others, were proper regulation of the patient's diet and the use of opiates and diaphoretics.

As one of the most prominent symptoms of the disease consists in the abnormal formation of sugar in the system, it would at once seem probable that advantage might be derived from the exclusion of all articles of food or drink that contain sugar or elements capable of being converted into sugar. To debar the patient from the use of such ingesta would appear the readiest and most effectual method of cutting off the supplies which the disease requires to enable it to carry on its warfare against the system.

Accordingly, the exclusive use of animal food has been prescribed, and all vegetable substances have been prohibited; and from the time of Dr. Rollo this has been generally considered the most important part of the treatment.

When a dietary of this kind is strictly enforced, it produces good effects almost immediately; and ill effects not very much later.

The good effects are marked by diminution of the quantity of sugar contained in the urine, and of the urine itself; by consequent relief of the thirst, of the hunger, of the dryness of the skin, and of the nervous irritability and restlessness; and by increase in the bulk and strength of the body, and in the serenity and cheerfulness of the mind.

These are most salutary changes; but, unhappily, they are in almost all cases speedily followed by great evils. The patient in a very short time begins to long for vegetable aliment; he acquires a distaste for animal food, and this speedily amounts to aversion and loathing; like the discontented Hebrews, he abominates quails, and remembers cucumbers, and melons, and leeks, and onions, and garlic. To prevent him,

when he has reached this condition, from violating your injunctions would require a degree of authority equal to that of the Baratarian medical adviser of Sancho Panza,—would demand more of care and precaution than can generally be used in a hospital, and far more than can ever be enforced in private practice.

In one of our cases the effect of animal diet was, in a few days, to excite so much of dysenteric irritation, as to render a change of aliment absolutely necessary. I have observed the same result in another instance.

We are compelled after a time, and generally after a very short time, to consent to the use of a certain amount of vegetable food. The vegetables allowed should be those which contain as little as possible of saccharine or amylaceous matter, such as spinage, celery and cabbage. But, with all care in selection, the change is always followed by alteration for the worse in the character of the urine and in all the symptoms of the disease.

The use of animal food may be favored by gratifying the patient with as much variety in its kind, and in the modes of preparing it, as he may desire. Those who can best and longest endure it have the best chance of recovery. Dr. Bardsley, whose remarkable experience has been referred to, ascribes his unprecedented success in the treatment of the disease to his strictness in enforcing the use of animal, and preventing that of vegetable, diet.

Whatever food may be employed, monitions should be given against the taking of it in too abundant quantity. The appetite is generally good, and patients often injure themselves by eating too freely.

Opium is, of all medical drugs, the one that has been most useful against diabetes. It is given for the purpose of diminishing the secretion of urine and of calming the distressing nervous irritability of the system. It possesses no power to change the saccharine condition of the urine. Combined with ipecacuanha, in the form of Dover's powder, it constitutes the most efficacious calming and diaphoretic remedy that we can prescribe.

Diaphoretics are recommended by their power to remove the dry and harsh state of the skin, to determine the fluids to the vessels of the surface, and thereby to lessen irritation in the kidneys and other internal organs. The best of them, after Dover's powder, are the hot-air and warm-water baths, friction with the flesh-brush, daily exercise, and the wearing, during all seasons, of flannel next to the skin.

Aperients are generally required throughout the disease, and should be selected from the milder agents of their class, and aided, if necessary, by enemata. Saline cathartics should be avoided from their tendency to increase the action of the kidneys.

Rennet, which was prescribed in one of our cases, but not used long enough to enable us to judge of its value, has been recommended from its power to convert a solution of sugar into lactic acid, and in the hope that what it does out of the body, it would be able to accomplish within the body. Experience, I believe, has said but little in its favor, and it appears to succeed better in the laboratory than in the sick-room.

The topical and general *abstraction of blood*, advised by many of the writers, I have never employed, nor seen cases in which they appeared to be either required or allowable. They are probably useful in the early stage of attacks attended by considerable febrile commotion; but would be injurious and dangerous when much debility has occurred.

Alkaline remedies have been suggested and employed in compliance with theoretic doctrines of doubtful character; but when long and freely used, they have often been found beneficial. Magnesia and carbonate of ammonia are the agents most commonly prescribed. The latter of these was given in one of our cases, and apparently with some degree of advantage.

Tonics are very generally indicated by the extreme debility of the patient, and the most eligible of them are the simple bitters and the preparations of iron. The latter are especially required by the anæmia, which, sooner or later, is always induced by the disease.

The statements, Gentlemen, which you have heard and read in relation to the pathology and treatment of diabetes must, I am aware, appear to you unsatisfactory and discouraging. You will be apt to consider our knowledge of the one far less accurate, our success in the other far less complete, than might be desired or reasonably expected. But you must make just allowance for the great and inherent difficulties of the subject. You must reflect on the narrowness and imperfection of all human science. And you may also find occasion for gratification and honest professional pride in comparing our knowledge of the natural history and proper therapeutic management of the obscure and intractable malady we have been considering with the knowledge possessed by our immediate predecessors. You will observe that if much remains to be done, much likewise has been successfully accomplished. The learned and experienced Dr. Heberden, who died in 1801, refers to no other method of ascertaining the presence of sugar in the urine than by the taste; and he appears to have regarded all treatment of diabetes as useless and hopeless. At the present day we are better prepared to detect and discriminate the disease; and if in most cases we are still unable to effect its cure, we can yet do so much to alleviate the miseries and to prolong the lives of those whom it attacks, that you can scarcely contemplate the result without finding additional reason to love and reverence the beneficent art whose study and practical application are to constitute the business, and I hope the pleasure, of all your future years.

ART. II.—*Ethnological Papers.* By W. S. FORWOOD, M. D.,
of Darlington, Maryland.

[No. I.]

“I shall not pretend that, in my own mind, I regard the question now to be discussed (unity of mankind) as one of which the decision is a matter of indifference either to religion or humanity. But the strict rule of scientific scrutiny exacts, according to modern philosophers, in matters of inductive reasoning, an exclusive homage. It requires that we should close our eyes against all presumptive and extrinsic evidence and abstract our minds from all considerations not derived from the matters of fact which bear immediately on the question. The maxim we should follow in such controversies is, ‘*fiat justitia, ruat cælum.*’ In fact, what is actually true it is always most desirable to know, whatever consequences may arise from its admission.”—PRICHARD’S *Natural History of Man*; 4th ed., edited by Edwin Norris; London, 1855, vol. 1, p. 7.

PREFATORY REMARKS.

Under the general head of “*Ethnological Papers,*” we propose presenting to the readers of the *Maryland and Virginia Medical Journal*, a series of articles, more or less extended, treating upon various branches of this interesting subject—Ethnology. It is not our intention to observe any special line of proceeding from one branch to another, but, with certain restrictions, to treat them all indifferently, as regards order, as circumstances and particular trains of thought may suggest; thus, in a large degree, giving these articles a disconnected and desultory character.

The study of the natural and physical history of man has hitherto been confined to comparatively a small number of individuals; and their observations have generally been recorded in the technical phraseology of the science; and hence, for the want of a more general diffusion and greater simplicity of language, the large body of ordinary or general readers, and the devotees of other sciences, have no very clear ideas of the present state of Ethnology; nor are they acquainted with the general arguments employed by parties discussing the

relationship of, or connection between, the different races of men. We desire to popularize the study of ethnology, as has been done with the cognate sciences, astronomy, geology, etc. All branches of knowledge advance in proportion to the number of students engaged in their prosecution.

This subject comes legitimately under the notice of medical men, as anatomists, physiologists, and observers of the phenomena of nature; whose range of studies embrace more departments of learning than that of any other profession. It is well known, however, that the majority of physicians are either too closely engaged in the practical duties of their profession, or feel too little interest in the study of ethnology, to make upon themselves the trouble and expense of reading the many volumes treating upon the subject. But there are, doubtless, many who would gladly avail themselves of the opportunity of reading an epitome of the reasonings and conclusions of various authors, in periodical articles, at a small cost of time and money, who would shrink from the perusal of a single volume on any one branch of natural history. These are our reasons for assuming the arduous undertaking; and we select this journal as an appropriate channel of communication.

SPECIES.

Naturalists divide animals, for purposes of description, into types, classes, orders, families, genera and species. Species is the last division between closely allied animals that is recognized in the systems of zoological classification. The term *varieties* is much used at the present day, particularly in speaking of the human genera, and of domesticated animals, to designate a sub-division of species, or to indicate the changes that take place in individuals of the same stock under the notice of man within a comparatively short period of time. We are at a loss, however, to understand how authors speak of *permanent varieties* simply as varieties in those cases where they have existed anterior to our certain knowledge; for *permanence of type* is regarded by some of our best authorities

as a special characteristic of *species*. We are unable to see the necessity for introducing terms into our classification which have such unsettled and doubtful meaning; since they tend only to confuse and mislead the mind of the student and retard the progress of science. Hence, in speaking of varieties, we prefer using the term in its common acceptation, and not as signifying the type of any species.

The term species has been variously defined. The following definitions, which are quoted by a number of recent writers on natural history, may be received as unexceptionable as any other; but, as Dr. Nott says, they fall far short of the true difficulties of the case:

“We are under the necessity of admitting the existence of certain forms which have perpetuated themselves, from the beginning of the world, without exceeding the limits prescribed: all the individuals belonging to one of these forms constitute a *species*.”—CUVIER.

“The name species is applied to an assemblage of individuals which bear a strong resemblance to each other, and which are perpetuated with the same essential qualities. Thus man, the dog, the horse, constitute to the zoologist so many distinct species.”—MILNE EDWARDS and ACHILLE COMTE.

These general definitions do not appear objectionable if we except the examples cited, which are disputed points, and are not received by many of the leading naturalists of the day.

“Species are fixed and permanent forms of being, exhibiting certain modes of variation, of which they may be more or less susceptible, but maintaining throughout those modifications a sameness of structural essentials, transmitted from generation to generation, and never lost by the influence of causes which otherwise produce obvious effects. *Varieties* are either accidental or the result of the care and culture of man.”—MARTIN; *Natural History of Man and Monkeys*.

The Rev. Dr. Bachman, of Charleston, gives another definition, substantially the same, to which he appends, as an additional test of species, the production of “*fertile offspring by association*.” This is assuming as a fact one of the prin-

principal points of dispute; and, if accepted, would settle at once the whole question of Hybridity, which is occupying at the present time so large a share of attention.

“*Varieties*,” says Dr. Bachman, “are those that are produced within the limits of particular species, and have not existed from its origin. They sometimes originate in wild species, especially those that have a wide geographical range, and are thus exposed to change of climate and temperature,” &c.

* * * “*Permanent Varieties* are such as, having once taken place, are propagated in perpetuity, and do not change their characteristics unless they breed with other varieties.”

As we have before remarked, the existence of such *permanent varieties* as are here described is a subject of dispute.

The same author continues:

“On comparing these definitions, as given by various naturalists, each in his own language, it will be perceived that there is no essential difference in the various views expressed in regard to the characters by which a species is designated. They all regard it as ‘the lowest term to which we descend, with the exception of *varieties*, such as are seen in domestic animals.’ They are, to examine the external and internal organization of the animal or plant—they are, to compare it with kindred species, and if by this examination they are found to possess *permanent characters differing from those of other species*, it proves itself to be a distinct species. When this fact is satisfactorily ascertained, and the specimen is not found a domestic species in which varieties always occur, presumptive evidence is afforded of its having had a primordial existence. We infer this from the fact that no species is the production of blind chance, and that within the *knowledge of history* no true species, but *varieties* only, whose origin can be *distinctly traced to existing and well-known species*, have made their appearance in the world. This, then, is the only means within the knowledge of man by which any species of plant or animal *can be shown* to be primordial. The peculiar form and characters designated the species, and its origin was a necessary inference derived from the characters stamped on it by the hand of the Creator.”*

* Not having Dr. Bachman’s book before us, we have made the foregoing extracts from Dr. Nott’s *Appendix to De Gobineau’s Diversity of Races*.

Dr. Nott adds to the above :

“To all the positions thus far taken by Dr. Bachman, we most cheerfully subscribe ; they are strictly scientific, and by such criteria alone do we desire to test the unity of the human family ; but we must enter a decided demurrer to the assertion which follows, viz : that ‘according to the universally received definition of species, all the individuals of the human race are proved to be of one species. When it shall be shown that all the races of men, dogs, horses, cattle, wolves, foxes, &c., are ‘varieties only, *whose origin can be distinctly traced to existing and well-known species,*’ we may then yield the point ; but we must be permitted to say that Dr. Bachman is the only naturalist, as far as we know, who has assumed to know these original types.”

The reader will perceive that the definitions of species cited are not based upon *anatomical characters*, but simply on the *permanency* of certain organic forms, and that this permanence of form is determined by its *history* alone.

Dr. Nott continues :

“Prof. Owen, of London, has thrown the weight of his great name into the scale, and tells us that ‘man is the sole species of his genus, the sole representative of his order.’ But proving that man is not a monkey, as the professor has done in the lecture alluded to, does not prove that men are all of *one* species, according to any definition yet received ; he has made the assertion, but has assigned no scientific reasons to sustain it. No one would be more rejoiced than ourselves to see the great talent and learning of Professor Owen brought fully to bear on this point ; but, like most naturalists, he has overlooked one of the most important points in this discussion—*the monumental history of man.*”

“Will Professor Owen or Dr. Bachman tell us wherein the lion and tiger—the dog, wolf, fox and jackal—the fossil horse, and living species—the Siberian mammoth and the Indian elephant, differ more from each other than the white man and the negro ? Are not all these regarded by naturalists as distinct species, and yet who pretends to be able to distinguish the skeleton of the one from the other by specific characters ?

“The examples just cited, of living species, have been decided upon simply from their permanency of type, as derived from their history ; and we may say that, by the same process

of reasoning, the races of men depicted on the monuments of Egypt five thousand years ago, and which have maintained their types through all time and all climates since, are a *distinct species*."

Dr. Morton, in one of his papers published in the *Charleston Medical Journal and Review*, briefly defines species to be "a *primordial organic form*," and regards all forms as primordial which have preserved their specific characters through all human records. This definition was adopted by Professor Agassiz. "Dr. Bachman objects very strongly to this definition," says Dr. Nott, "and declares it a 'cunning device, and, to all intents, an *ex post facto* law, suddenly conjured up during a controversy, to avoid the difficulties of the case; but we have serious doubts whether these gentlemen are capable of such subterfuge in matters of science, and confess that we cannot see any substantial difference between their definition and those given by Dr. Bachman. Morton and Agassiz determine a form to be '*primordial* by its permanency, as proved by history, and the other definitions assign no other test.'"

Dr. Cabell, in his work on the "*Unity of Mankind*," after quoting Dr. Bachman to show that Morton's definition of species is a "cunning device," &c., goes on to give the following *lucid* definition from Prof. Dana of Yale College: "A species among living as well as inorganic beings is based on a *specific amount or condition of concentrated force defined in the act or law of creation*." *

In a foot-note the writer expresses his surprise that many of the readers of the first edition of his book should "have experienced some embarrassment in relation to the significance of Prof. Dana's above definition of species."

Prof. Leidy's definition is as complete, perhaps, as could be given in a few words, viz:

"A species is a mere convenient word, with which naturalists empirically designate groups of organized beings possessing characters of comparative constancy, as far as historic

* P. 33. The italics are his.

experience has guided them in giving due weight to such constancy.”—*Proceedings of the Academy of Natural Sciences*, vol. vii, p. 201.

We might quote numerous other explanations of the term species from various well known authors, but as they all appear to have nearly the same leading idea, we will confine our subsequent remarks on this subject to one or two recent works by high authorities.

Prof. Agassiz has treated the whole subject in an exhaustive manner in his “*Essay on Classification*,” contained in the first part of the first volume of his great work entitled “CONTRIBUTIONS TO THE NATURAL HISTORY OF THE U. STATES.” He does not attempt to define species in a single paragraph, as in the foregoing instances, but the section he has devoted to its consideration occupies several pages; we must, therefore, be content with giving a brief synopsis of a part of his views. He begins by remarking that it is generally believed that nothing is easier than to determine species; and that “an unfailing criterion of specific identity is even supposed to exist in the sexual connection which so naturally brings together the individuals of the same species in the function of reproduction. But I hold that this is a complete fallacy, or, at least, a *petitio principii*, not admissible in a philosophical discussion of what truly constitutes the characteristics of species. I am even satisfied that some of the most perplexing problems involved in the consideration of the natural limits of species would have been solved long ago, had it not been so generally urged that the ability and natural disposition of individuals to connect themselves in fertile sexual intercourse was, of itself, sufficient evidence of their specific identity.”

Without entering into the laws of hybridity, he says: “I will only remark that so long as it is not proved that all the varieties of dogs, and of many other of our domesticated animals, and of our cultivated plants, are respectively derived from one unmixed species, and as long as doubts can be entertained respecting the common origin of all the races of men from one common stock, it is not logical to admit that sexual connection

resulting even in fertile offspring is a trustworthy evidence of specific identity."

To justify this assertion, he asks if there is an "unprejudiced naturalist who in our days would dare maintain: 1st, that it is proved that all the domesticated varieties of sheep, of goats, of bulls, of llamas, of horses, of dogs, of fowls, etc., are respectively derived from one common stock; 2d, that the supposition that these varieties have originated from the complete amalgamation of several primitively distinct species is out of the question; and 3d, that varieties imported from distant countries and not before brought together, such as the Shanghae fowl, for instance, do not completely mingle?" etc. He considers that the test of hybridity is of no practical value in the cases of highest scientific interest, and believes that accurate science must do without it; and concludes that the sooner it is altogether discarded the better. "But," he adds, "like many relics of past time, it is dragged in as a sort of theoretical bugbear, and exhibited only now and then to make a false show in discussions upon the question of the unity of the origin of mankind."

He contends that species exist the same way in nature as genera, families, orders, classes, and types. Further on, speaking of the doctrine of the unity of origin of the human species, he says: "For my own part, I cannot conceive how moral philosophers, who urge the unity of the origin of man as one of the fundamental principles of their religion, can at the same time justify the necessity which it involves of a sexual intercourse between the nearest blood relations of that assumed first and unique human family, when such a connection is revolting even to the savage."

It may not be improper to state in this connection *our* doubts (not suggested by Prof. A.), founded upon the physiological laws of reproduction, as to the possibility of a single pair propagating a permanent species; but it would be manifestly out of place to introduce any argument here on this subject.

"*As representatives of species,*" continues Prof. A., "individual animals bear the closest relations to one another; they

exhibit definite relations also to the surrounding elements, and their existence is limited within a definite period."

Again: "All the individuals of any or of all species now existing, are only the successors of other individuals which have gone before, and the predecessors of the next generations; they do not constitute the species—they represent it."

Prof. A. still adheres to the belief, long since announced by him, that all species were originally created in greater numbers than a single pair; and, to any unprejudiced mind, it appears to us that this view must be regarded as the most reasonable.

To sum up the idea that at the present day is generally understood by naturalists when speaking of species, it may be simply stated that species are the direct representatives, or lineal descendants of original creations; and they are known to be such by the possession of certain more or less permanent characters, which they have maintained through all historic time.

What has occurred anterior to human records, can only be inferentially determined by subsequent experience, upon the same principle by which, with our knowledge of the immutability of the laws of nature, we predict the future by the past.

The distinguishing or specific marks by which a particular species may be recognized, will command our attention at a future time.

Having now given what may be regarded as the generally received views upon this interesting branch of natural history, (species,)—which, since the days of Aristotle, has occupied so large a share of the attention of many distinguished men, from the numerous difficulties presented in many instances to the positive decision upon particular forms—we propose to draw attention to a new work on the subject, by Mr. Darwin, entitled "*The Origin of Species*."* As this remarkable work has only

* *On the Origin of Species by means of Natural Selection, or the Preservation of Favored Races in the Struggle for Life.* BY CHARLES DARWIN, M. A., Fellow of the Royal, Geographical, Linnæan, etc. Societies; Author of "Journal of Researches during H. M. S. Beagle's Voyage Round the World." New York. 1860.

been before the British and American public for a very short time, we presume that the great body of our readers have not yet perused it, or perhaps seen more than its title named in the periodical papers of the day, for we are not aware that more than the briefest notice of it has yet been published in this country: therefore, we propose on the present occasion taking somewhat extended view of the author's peculiar opinions, and his arguments supporting them.

We may state in advance that the author's reasoning and conclusions are highly ingenious, and, we must say, quite plausible. They are at least entitled to our most respectful consideration, as they come from a man of established reputation, are presented, not in a controversial spirit, but in a calm and philosophic style appropriate to the dignity of the subject, and are the results of many years' close and patient study. The author states, however, that the present volume is merely an abstract of a larger and more complete work that he has been engaged upon for a considerable time, and which may yet require two or three years to finish. He has been induced to give this instalment in advance from the fact that another naturalist, (Mr. Wallace,) who is now studying the natural history of the Malay Archipelago, has arrived at almost exactly the same general conclusions respecting the origin of species.

As stated in the title of this work, the author maintains the view that species arise from natural selection, or from the perpetuation of variations which naturally occur, which it is supposed are induced more from the necessities of surrounding circumstances than from mere accident. As, for example, the flying beetles that inhabit oceanic islands gradually lose the use of their wings, from the fact that all of those that possess these organs in a perfect state are blown away from the land by the winds while flying, and are lost in the water, while upon those with less perfectly developed wings, or of more indolent habits, devolves the office of reproduction; their progeny partake, to some extent, of the imperfections of their parents; the best flyers of each generation perish like their predecessors from being carried by the winds beyond the limits of the island, until

finally only those that are unable to fly remain, their wings having been reduced, by this process of "natural selection," to a merely rudimentary state. Thus a flying species of insects are changed, by extraneous circumstances, into what naturalists would most likely class as a distinct species, for the capacity of flying is undoubtedly a specific distinction—at least, according to all our former rules. Mr. Wollaston (quoted by our author) vouches for the fact that the changes mentioned do actually occur in numerous species of beetles in Madeira from the operation of the causes above assigned.

Thus we have at the outset, in the foregoing example, given one of the author's leading ideas, which is advanced in the body of the work to show the effects of "use and disuse," in a few words, viz: that species are not types of original creation, but the products of a succession of slight changes continued through numberless generations.

In the Introduction (p. 13), the author proclaims his settled convictions in the following words :

"Although much remains obscure, and will long remain obscure, I can entertain no doubt, after the most deliberate study and dispassionate judgment of which I am capable, that the view which most naturalists entertain, and which I formerly entertained—namely, that each species has been independently created—is erroneous. I am fully convinced that species are not immutable; but that those belonging to what are called the same genera are lineal descendants of some other and generally extinct species, in the same manner as the acknowledged varieties of any other species are the descendants of that species. Furthermore, I am convinced that natural selection has been the main but not exclusive means of modification."

Chapter the first treats of the variations of species—plants and animals—that occur under the state of domestication. He says that there is no case on record of a variable being ceasing to be variable under cultivation. Plants that have been cultivated for many ages, such as wheat, still often yield new varieties; and our oldest domesticated animals are still capable of rapid improvement or modification. Hence we are led to infer

that the continuance of the changes through hundreds of centuries to come will transform our present domesticated plants and animals to a nature as essentially differing from what they now are, as they now differ from what they were as many centuries past. In this chapter our author expresses his opinion that all our dogs have not descended from any one wild species (viewing species as they now exist): thus opposing a favorite theory and one of the strongest props of Prichard and other unitarists, in support of their doctrine that unlimited prolificacy *inter se* is indubitable evidence of a common origin. If it can be proven that our domestic dogs have originated from various species, hybridity will no longer be regarded as a test of species; and hence the proper consideration and classification of the human species will be relieved of its greatest embarrassment.

While contending that there exists great variation in the species of dogs, as in other domesticated animals, Mr. Darwin freely admits his belief in their distinctness of origin, or rather distinctness of ancestry through many ages.

On this subject he says:

“Even in the case of the domestic dogs of the whole world, which I fully admit have probably descended from several wild species, I cannot doubt that there has been an immense amount of inherited variation. Who can believe that animals closely resembling the Italian grey-hound, the blood-hound, the bull-dog, or Blenheim spaniel, &c.—so unlike all wild Canidæ—ever existed freely in a state of nature? It has often been loosely said that all our races of dogs have been produced by the crossing of a few aboriginal species; but by crossing we can get only forms in some degree intermediate between their parents; and if we account for our several domestic races by this process, we must admit the former existence of the most extreme forms, as the Italian grey-hound, blood-hound, bull-dog, &c., in the wild state. Moreover, the possibility of making distinct species by crossing has been greatly exaggerated. There can be no doubt that a race may be modified by occasional crosses, if aided by the careful selection of those individual mongrels, which present any desired character; but that a race could be obtained nearly intermediate between two extremely different races or species, I can hardly believe. Sir J. Sebright expressly experimentized for this object, and

failed. The offspring from the first cross between two pure breeds is tolerably and sometimes (as I have found with pigeons), extremely uniform, and everything seems simple enough; but when these mongrels are crossed with another for several generations, hardly two of them will be alike, and then the extreme difficulty, or rather utter hopelessness of the task becomes apparent. Certainly, a breed intermediate between *two very distinct* breeds could not be got without extreme care and long-continued selection; nor can I find a single case on record of a permanent race having been thus formed." Pp. 24, 25.

The author dwells at considerable length on the subject of domestic pigeons. He believes that they all, the carrier, the short-faced tumbler, the common tumbler, the runt, the barb, the pouter, the turbit, the jacobin, &c., have descended from the rock-pigeon (*Columbia livia*), which term, he says, includes several geographical races or sub-species, which differ from each other in the most trifling respects.

The remarkable changes produced in the characters of the domestic pigeons by the process of *artificial* selection affords our author a powerful argument in favor of his views upon the continuous production of species by the transformation of kindred types.

"Altogether at least a score of pigeons might be chosen, which, if shown to an ornithologist, and he were told that they were wild birds, would certainly, I think, be ranked by him as well-defined species. Moreover, I do not believe that any ornithologist would place the English carrier, the short-faced tumbler, the runt, the barb, the pouter, and fan-tail in the same genus; more especially as in each of these breeds several truly-inherited sub-breeds, or species as he might have called them, could be shewn him." P. 27.

He proceeds to explain how slight variations in the original stock may, by accidental or intentional selection, gradually assume exaggerated forms; but our limits will not permit us to accompany the author further in this direction. Suffice it to say, that in the main his arguments on this point appear to be founded on facts.

The author acknowledges that some domestic animals vary much less than others; for instance, the rarity of absence of distinct breeds of the cat, the donkey, peacock, goose, &c.; but he explains this persistent distinctness by asserting that selection in these cases has not been fully brought into play, for the reasons, viz: "In cats the difficulty of pairing them; in donkeys, from only a few being kept by poor people, and but little attention being paid to their breeding; in peacocks, from their not being very easily reared, and a large stock not being kept; in geese, from being valuable only for two purposes, food and feathers, and more especially for no pleasure having been felt in the display of distinct breeds." P. 44.

These reasons for the stability of the above named species appear to us quite insufficient and unsatisfactory. We admit that there is some difficulty in pairing cats, but it is by no means insuperable. We can with cats as easily exclude those that we do not wish to breed from, as in the case of pigeons, dogs, &c.; but why do not varieties of these animals occur *naturally*, thus affording a starting point for the cultivation of distinct breeds, as in those species in which the greatest changes are observed? A few pages back (p. 41) our author expressly says, that man "*can never act by selection, excepting on variations which are first given him in some slight degree by nature*;" he cannot form any character he may desire, but may take advantage of variations or abnormities that accidentally present themselves, and by careful inter-breeding between these peculiar individuals, may increase their new characters until they form a distinct variety. Does not a tendency to change occur naturally in cats? If it does not, the supposed law cannot be claimed as having universal application. His reasons for the permanent characters of the donkey, peacock and goose are equally objectionable. Donkeys are not very numerous, in comparison with some similar animals, it is true; it is also admitted that they are generally kept by the poorer class of people, and that comparatively little attention is paid to their breeding; but we cannot conclude with the author, however, that their numbers are so few

as to preclude their liability to change or variation (if number be necessary to that result); for in some countries they are quite abundant—much more so than horses—and are much prized for their sure-footedness in climbing mountains, and for their great power of endurance,—performing long journeys heavily ladened, when but scantily supplied with food,—surpassing in these respects the nobler animal, the horse. The fact that these animals are generally in the hands of the poor classes can scarcely be advanced as an argument for their remaining unchanged. This class could not of course afford to experiment extensively on theoretical grounds, upon the doubtful assumption of producing a particular result; but it is a very reasonable supposition that they would take advantage of any *natural* variation that would be either useful or ornamental. And as to their being but little attention paid to their breeding, is not this rather the result than the cause of their permanence?—breeders not receiving any encouragement in their attempts to effect change or improvement?

In the case of peacocks he tells us that they are permanent because they are not easily raised, and a large stock is not kept. We have known them to be reared in considerable numbers by persons who prized them for their beauty; and, as with the donkey, we think they are sufficiently numerous for the operation of the natural laws that bring about the supposed variations in species. Geese do not vary because they are “valuable only for two purposes, food and feathers, and more especially from no pleasure having been felt in the display of distinct breeds!” This argument needs no comment. The guinea-fowl is not mentioned; it is as unvarying as any of the species we have referred to; and as it is useful for the same purposes for which we value the common barn-door fowl,—viz., for food and eggs,—according to the general law announced we would naturally expect a like degree of variation.

Without dwelling longer on the changes that are observed in some domesticated animals, we will proceed to the notice of chapter second, the subject of which is, “*Variation under nature.*”

This is certainly the legitimate sphere for observation in the investigation of this subject; but the author does not present any particular instances, reserving these cases, we presume, for discussion in his more complete work. He speaks of variability, individual differences, and doubtful species; he also states that wide ranging, much diffused, and common species vary most; that species of the larger genera in any country vary more than the species of the smaller genera, and that many of the species of the larger genera resemble varieties in being very closely, but unequally, related to each other, and in having restricted ranges.

"*Struggle for Existence*," forms the heading of the third chapter.

All organized beings have a natural tendency to increase much beyond the number that it is possible to survive in the struggle for life. Those that do survive are probably enabled to support life by the possession of some powers superior to those of their fellows.

"Owing to this struggle for life" (says our author), "any variation, however slight and from whatever cause proceeding, if it be in any degree profitable to an individual of any species, in its infinitely complex relations to other organic beings and to external nature, will tend to the preservation of that individual, and will generally be inherited by its offspring. The offspring, also, will thus have a better chance of surviving, or, of the many individuals of any species which are periodically born, but a small number can survive." P. 61.

This principle, by which each slight variation, if useful, is preserved, is termed by the author, Natural Selection, in contradistinction to artificial or man's power of selection.

The author asserts (p. 63) that there is no exception to the rule that every organic being naturally increases at so high a rate, that if not destroyed, the earth would soon be covered by the progeny of a single pair. Upon an accurate calculation, at its probable minimum rate of natural increase, the elephant, which is reckoned to be the slowest breeder of all known animals, would produce in five centuries fifteen million of its species from a single pair.

Some animals are undoubtedly created especially to prey upon others, and thus prevent an injurious increase in their numbers; and upon the abilities of the weak, either natural or acquired, to escape from their enemies, will depend the rate of their multiplication—not by the number of animals born, or of eggs laid, as might naturally be supposed,—for it has been ascertained, for example, that the Fulmar petrel lays but one egg, yet it is believed to be the most numerous bird in the world. “One fly deposits hundreds of eggs, and another, like the hippobosca, a single one; but this difference does not determine how many individuals of the two species can be supported in a district.” P. 65.

There are many checks to the growth of species, such as unfavorable conditions of climate, scarcity of food, &c., in addition to their liability to destruction by other species, which are dilated upon by our author at some length.

The increase of plants is regulated by similar laws.

(To be concluded in next number.)

ART. III.—*An Analysis of a Case of Pseudo-Syphilitic Cachexia.* By B. ROEMER, M. D.

1856, July 21.—Mr. —, æt. 45, was treated two years ago for chancre,—was then profusely salivated. Said that he suffered much from phimosis. His physician failing to aid him as he expected, he applied to another, who again mercurialized him; lanced the prepuce, and recommended a wash, which, on having tested it, proved to be the common black-wash. Symptoms of bubo were treated with success. Four months afterwards, (about April, 1855,) ulcers appeared upon the left elbow-joint, both tibia, about two inches above its articulation with the astralagus, and in the nasal cavity (left side.) Subsequently, nodi of considerable basis formed upon both legs

(tibia), and immediately below the patella. Dimensions as follows:

Left tibia—longest diameter 6.5; shortest diameter 4; circumference 8.5; perpendicular radius 1.5 inches.

Right tibia—longest diameter 5.5; shortest diameter 3.5; circumference 7; perpendicular radius 1 inch.

Pains distressing. The right testicle is hypertrophied. The ulcers commenced in a papulous eruption of the size of a pin's head, forming continually over a space of from five to six square inches, which created a scab, under which ulceration continued until the whole assumed the character of large, flat bullæ. The adjacent skin bears the characteristic red-brown color. Experiences, often, vertigo and a sense of heat and pains on the side of the head. Has rheumatic pains in the shoulders and joints, with stiffness especially in the morning. At times, obstinate sore throat supervenes; voice husky; complexion coppery, otherwise to appearance healthy.

The unilateral affection of the skeleton is not present in this case, but perhaps the symmetrical:

Left Side.

Right Side.

Elbow (ulcers.)

Tibia (ulcer and node).

Nasal cavity (ulcer).

Nasal bone (node).

Patella (node).

————

————

Shoulder (rheumatic pain).

Finger-joints (pain and stiffness.)

————
Tibia (ulcer and node).

————

————

Patella (node).

Testicle (hypertrophy).

Os temporal, os parietal, and towards occiput (pain).

Shoulder (rheumatic pain).

Finger-joints (pain and stiffness).

His pulse never ranged over 90 to the minute, moderately full. His weight about 180 pounds. The growth of hair is thin and coarse.

The treatment consisted in revellents applied upon the ulcers. As soon as free suppuration was established, cold water in jets was allowed to fall upon the affected parts (nodes

included) from a height of four feet. Around the ulcers Tr. iod. s. was painted, and

R—Tr. iod. s. f3jss.,
Pot. iod. ʒij.,
Dec. sarsap. f3vi.

A tea-spoonful three times daily, gradually increased to a table-spoonful; the former dose then to be resumed after an interval of five days. Dieted him strictly. In the fourth week, the rheumatic pains were somewhat relieved, as also the tenderness of the nodes. The cavities under the bullæ granulated. Iodine was brought in contact with the ulcer in the nose by means of a small glass brush. The nodes were arrested in their growth—that of the left tibia even receded—forming a hollow with elevated edges. The spinous process marked. In three months, the ulcer has cicatrized, and remained so; the redness around vanished.

Feb. 20, 1857.—The pain of the 'nodes rather increased. Ulcers still closed. The nodes present the following dimensions:

The right tibia has a perpendicular radius of 0.8 inches; hard to the touch. The left tibia shows only a semi-circular row of hard and interrupted elevations. The node under the patella of former size.

Applied Collodion f3i.,
Iod. gr. xx. s. over the nodes.

Refused narcotics to the extent of the patient's wish. The ulcer of the nose has healed.

The patient having suffered on March 16, 1857, a fall, producing slight concussions, with some effusion of blood from the knee, I instituted the following analysis for iodine in the blood, urine, saliva and sweat, which was repeated at three different dates:

	<i>Urine.</i>	<i>Saliva.</i>	<i>Sweat.</i>	<i>Blood.</i>	<i>Test.</i>
I.—March 17, 1857,	0.50	1.55	0.20	0.05	Amylum. Nitrate of silver.
April 28, 1857,	0.35	0.90	0.30	—	
November 10, 1857,	0.70	0.55	0.05	—	
January 20, 1858,	0.20	1.95	0.25	—	
Average in 1.000 parts,	0.4375	1.2375	0.2000	—	

	<i>Urine.</i>	<i>Saliva.</i>	<i>Sweat.</i>	<i>Blood.</i>	<i>Test.</i>
II.—March 17, 1857,	0.35	1.25	0.15	—	} Bichlor. of Palladium.
April 28, 1857,	0.30	0.85	0.20	—	
November 10, 1857,	0.55	0.50	0.05	—	
January 20, 1858,	0.15	1.60	0.20	—	
Average in 1.000 parts,	0.3375	1.0500	0.1500	—	

N. B.—It appears that the second cotemporary analysis proved itself far from being accurate, owing perhaps to the impurity of the salt. The increase of iodine in the saliva and sweat was in a direct ratio with its functional effects upon the system, whilst the renal organs eliminated it to a greater extent when its action was latent or regulated through a uniform absorption. Iodism appeared only in form of slight salivation from January 19–21, 1858, which subsided without interference.

We deem it of sufficient importance to add here a brief sketch of the manner of proceeding to detect iodine through chemical analysis, especially since text-books on organic chemistry never enter into specified details. We will premise that iodine passed off by the kidneys undergoes decomposition, it appearing with the usual salts of the urine as a hydriodate. And let us remark here, that this chemical process favors the therapeutic action of iodine succeeding mercurialization, since it is fully decomposed by mercury, leaving (prot) oxide of mercury and hydrogen. The saliva, coming in direct contact with iodine, forms a salt of potassium; if, however, the process is the result of secretions containing the non-metallic element, it appears in form of mucus, stained and in union with osmazome.*

In the sweat, iodine may appear in union with osmazome or lactates. It should, however, be remembered, that those compounds, passed off by a certain secreting organ, are likely to be stimulants of that organ (Müller's Physiology); that, consequently, the ratio of elimination depends in a certain degree

* Vide Treviranus, *Biologies*, vol. iv.; Van Setten, *dissert. de Saliva ejusque vi et Utilitate*; Müller's *Arch.*, 1838, p. clxiv.

upon the chemical and organic action of the compounds upon the system. (See Tiedemann, Wochler's research. in Zeitschrift für Physiol. S. 2 and B. i.) The interstitial absorption (whether through the blood-vessels or lymphatics) of iodine shows but little evidence in the blood. Its presence is the more doubtful, since an external admixture of iodine (it having been painted over the skin where the blood was taken up for analysis) cannot be *strictly* prevented, although great care was taken to avoid it.

The substance to be analyzed having been treated with alcohol or ether, the solution was carefully separated from the residuum, and the ether evaporated. The dry sediment was resolved in a small quantity of water, and tested in the usual manner. The starch was prepared as a liquid, and the disproportion between the re-agent and the chemical gave only a dark green color, instead of blue. Upon application of heat to about 150° F. this color disappears, but returns on cooling, unless the liquid was brought to its boiling point. Chlorinated water destroys the blue color of iodine upon starch, but a careful addition of water impregnated with sulphuretted hydrogen, sulphurous acid, phosphorous acid, etc., reinstates it. Having thus defined the actual presence of iodine, a solution of nitrate of silver of given weight was added to a part of the liquid (observing always the usual rules of quantitative analysis); this process, however, having been found to give some free iodine instead of iod. silver, the silver oxide was first introduced, and afterwards nitric acid. If the acid is somewhat diluted, no disadvantages will result. The yellowish iod. of silver is dried and its weight determined. Its equivalent of iodine is calculated in the usual manner. The test with the salt of palladium results in a black deposit. Both the nitrate and chlorate will effect it. The solution is permitted to stand for 24 hours, since the deposit is but slowly forming. Wash the residue with hot water and filter. If the nitrate of palladium has been employed as a test, heat first the whole, and then filter. Also abstain from using ether as a solvent. Dry under 160° F., and equate the amount of iodine from the

pound weight of protiod. of palladium. The heat (160° F.) should not be exceeded, since otherwise a part of the iodine of the pallad. protiod. would be volatilized.

May, 1858. The patient remains *in statu quo*. No ulcer present. Nodes of former size, as Feb. 20, 1857. Pains periodic and very great. Is subject to deafness. Pupils at times contracted. Mr. — left for another city, and I saw no more of him until April, 1859. Has a vacant stare, is absent-minded, deafness at times complete. Complexion pale; over the sternum fiery red. Coughs. Weighs about 150 pounds. Was taken in April with total deafness, lasting three days. In one hour a complete change took place. Deafness altogether removed in about 30 minutes. Symptoms of pneumonia. Pulse low, typhoid. Continued rational for two days. Then slight delirium, restlessness. Continued to sink, and died seven days after the first attack of deafness—undoubtedly a symptom of abscess of the mastoid cells.

The patient had been treated, while absent, for the pain of the nodes with narcotics. In how far this may have affected the functions of the brain, I cannot judge, the time having been too short since his arrival. Post-mortem refused.

In conclusion, we would refer to the unusual metastasis of dysecoia. The total deafness existing for days admitted of no explanation that might be satisfactory; for, in spite of the writings of Deleau, Itard, Kramer, etc., we have not been able to come to proper conclusions. There existed no otorrhœa; the tympanum was found entire. The *sudden* removal of dysecoia is the most important point; and the usual causes of this affection do not admit of any aid. The mastoid cells were suspected of inflammation months before the patient died; a retrograde movement could not induce total deafness, followed in a few minutes by acute hearing. The only view, in its possibility best deciphering such a succession, is the following:

There existed, always, more or less ulceration of the throat; the nares had been inflamed. As in scarlatina, an extension of the morbid actions caused a pressure upon the pharyngeal opening of the Eustachian tube. This we consider the pri-

mary condition. We indicated above the almost constant narcotism imposed upon the patient for the relief of pain, etc., how the eye gave evidences of its results. The vice of the system, assisted, perhaps, by tonics, laid hold of the brain as the future material of destruction, and the portio mollis of the seventh pair became, by irritation, compressed in its cranial or auditorial course, thus completing dysecoia. The supervention of active inflammation of the lungs, (at least so at first,) diverting the direction of morbid changes, relaxed the force of a primary affection, the new centre of circulation restored the congested minor points of morbid action to or below the former condition in a direct ratio with the force of beginning inflammation, thus relieving the compressed orifice of the Eustachian tube, and stimulating the before paralyzed nerve.

ART. IV.—*Surgical Reports from the Record of the Infirmary of the Medical College of Virginia.* By THOMAS L. HUNTER, Jr., M. D.

No. I.—*Wound of Abdomen—Epigastric artery cut—Recovery.*—William S——; white man; age ——; in a fight a few hours ago, was stabbed with a short dirk in the abdomen. The knife entered about an inch and a quarter to the left of the median line, a little below the level of the anterior superior process of the ilium. Cutting inwards and upwards diagonally across the fibre of the rectus muscle—dividing a branch of the epigastric artery, and entered the abdominal cavity. The withdrawal of the knife was followed by considerable hæmorrhage, but this was soon arrested by a piece of the omentum as large as the hand, being forced through the wound during the efforts at vomiting and coughing, which immediately succeeded the injury, and acted as a natural hæmostatic.

When brought to the Infirmary, re-action had partially taken place. Dr. A. E. Peticolas, Attending Surgeon, re-

turned the escaped omentum, after having removed from it all foreign matter, to the abdominal cavity. And after satisfying himself, by an examination with the finger, that the intestines were not cut, he closed the wound with sutures, covered the abdomen with a wet cloth, placed the patient upon his back, with his thighs flexed upon his body, and supported with pillows. *Ordered*—Morphiæ sulph. gr. j.; to be taken at once, and to be repeated in six hours if restless and in much pain.

July 5th. Doing well. Has no fever, but slight pain and soreness in the abdomen. Complains of acute pain about the region of the heart. Bowels unmoved. Urine is of a dark red color, as if mixed with blood, causing pain when passed. It contains a large amount of albumen. But no blood corpuscles can be discovered under the microscope. He is very nervous and restless. *Ordered*—Morphiæ sulph. gr. ss. this morning, and to be repeated at bed time.

July 6th. Pulse natural. Skin cool and pleasant. Tongue clean. Slept well last night. Feels easier this morning than he has since accident occurred. Urine very dark-colored, and loaded with albumen. Bowels still unmoved. *Ordered*—Morphiæ sulph. gr. ss.; to be taken at bed-time. Given a few spoonfuls of soup.

July 7th. No material change in his condition since yesterday. *Ordered*—Morphiæ sulph. gr. ss.; to be taken at bed-time.

July 8th. Pulse a little weak, but perfectly quiet and natural. Pain very slight and confined to left iliac and lumbar region. Condition of the urine is unchanged. *Ordered*—Diminish dose morphiæ sulph. one-half. Give a quarter of a grain at bed-time.

July 9th. Improving. Had two large and healthy operations from the bowels during the night. They were passed without pain. Stop the morphine.

July 10th. Continues to improve. Slept well last night without an anodyne. Bowels opened again twice, passage natural. Urine lighter colored, contains very little albumen,

and is passed without giving pain. Given a large quantity of soup.

July 13th. With the exception of being very much debilitated, from the loss of blood at the time of the accident and the strict antiphlogistic regimen observed, he is entirely well. Has very little soreness about the abdomen, even on pressure. Bowels regular. Urine normal in color, and free from albumen. Tongue clean. Appetite good. In a few days will be allowed to walk about his room. Give solid, nutritious food in small quantity.

No. II.—*An operation for Removal of the Superior Maxillary bone.*—Alfred McC—; aged 40; abode, Albemarle co.; employment, farmer. Previous health and habits good. Admitted May 30th, 1860. Disease, osteosarcoma of upper jaw.

The superior maxillary bone of the right side is diseased and very much enlarged. It has been five years since he first experienced any uneasiness in the part—prior to that time he had suffered much from several decayed teeth.

The tumor formed by the diseased bone, extends from the middle incisor tooth to the posterior angle of the jaw bone. Projecting forward, it obliterates almost entirely the anterior nares of the right side, and so distends the upper lip as to prevent the perfect closure of the mouth. It also encroaches to some extent upon the hard palate, displacing the tongue. His general health is good. He has always been a hard-working man.

June 1st. At one o'clock, P. M., the diseased bone was removed by Dr. A. E. Peticolas. An incision was made from the angle of the mouth, running in a curved direction to a point just below the level of the external angle of the eye, and about an inch and a half behind it. The upper flap was dissected from the bone and the arteries tied. The flap being thrown back and held out of the way by an assistant, the bone was removed by means of Hey's saw and a stout pair of bone-pliers.

The orbital plate of the superior maxilla and the palate bone, not being diseased, were left uninjured.

The bleeding from the deep wound was slight, and easily controlled with persulphate of iron. After all hæmorrhage had ceased, the vacant space was filled with a sponge; and over that the flap replaced. Both edges were then brought together and retained by the interrupted suture and adhesive strips.

Complete anæsthesia was produced by chloroform before the operation began, but, owing to the position of the part, it was impossible to administer chloroform while the operation was going on, and it was only during an occasional pause in the operation that he could be allowed to inhale it. He was kept sufficiently under its influence to render him partially unconscious of his sufferings.

June 2d. Doing very well. Had a slight hæmorrhage from the nose last night, which was readily checked by injecting with a saturated solution of alum. Has some fever. Drinks a little milk from a physic-spoon. *Treatment*—Keep perfectly quiet, and leave the wound unmolested.

June 3d. The sponge was removed this morning; a slight hæmorrhage followed it. The cheek and eye-lid are much inflamed and swollen. Pain very little. Appetite good. Pulse quick and strong. *Treatment*—Wash out the mouth with tepid water and a weak solution of chlorinated soda. Apply wet lint to the external wound.

June 4th. Improving. Bowels costive. Tongue a little furred. Pulse weak, but more natural. Continue the same dressing. Give an enema of salt and water. And give him beef essence and brandy freely through the day.

June 5th. The enema produced several copious stools. Face easier and not so much swollen. Pulse better. Continue the previous treatment.

June 6th. The sutures were removed to-day, and the incision found to have united from the first intention.

On the 10th of June he was up walking about his room. On the 16th he was discharged and allowed to return home, having so far recovered as to need no further treatment.

ART. V.—*Operating Chair, for Surgical and Obstetrical Operations and Vaginal Examinations.* By CHRISTOPHER JOHNSTON, M. D., Baltimore, Md.

THIS Chair, as represented in the figure, was designed for the use of the Union Protestant Infirmary, and it has answered its purpose so well after a long trial as to have found favor with many members of the medical profession, including the Baltimore editor of the Maryland and Virginia Medical Journal.

It consists of a frame supported upon four legs; of a shallow stuffed seat, and a stuffed back which may be elevated and maintained at any angle by means of a frame and ratchet; of two bars, bearing foot-rests, united by cross pieces, and thus coupled, sliding in and out of a groove in the body of the machine, or removable entirely; and of a step working on pivots so that it may remain horizontal, or be placed vertically out of the way.

The Chair recommends itself by reason of its convenience, its simplicity, small size and price. When the back and seat are covered with a dark moroccoed canvas its appearance is not disagreeably suggestive in an office, for the canvas is readily freed from blood stains, &c.; and we feel assured that it will prove a great acquisition to the practitioner, either in private consultation or in hospital practice.

Dimensions: Length of frame, 3 feet 7 inches; width, 18 inches; height of the seat, 2 feet 8 inches, so as to allow the operator to be seated when exploring the vagina; depth of seat, 8 inches; length of back, 2 feet 9 inches.

Those Chairs now made and in use were manufactured by A. S. Jenkins & Co., walnut stuffed with hair, at a cost of \$19.

ORIGINAL TRANSLATIONS.

I. *Ether in the Treatment of Deafness*. By Dr. TRIQUET.
Translated from the *Journal des Connaissances Medicales* of
June 10, 1860.

Multa renascentur, quæ jam cecidere, cadentque,
Quæ nunc sunt in honore.—(HORACE.)

IF the value of things is to be judged of by the manner in which they are noised abroad, there would be reason to believe that the press has brought before the public a truly heroic remedy for deafness. For some time we have heard nothing spoken of but the marvellous cures wrought by the unfortunate young lady—Mlle. Cléret. Every one proclaims the merit of the poor girl to be in truth above all eulogy, since she has not hesitated to carry her self-devotion so far as to become a martyr to her own discovery, and in seeking to test its virtues, has experienced its dangers.* We should have been gratified if the medical committee appointed to observe the experiments of Mlle. Cléret had made known their opinion on the following questions: 1st. Whether the employment of ether, and of excitants in general in the treatment of deafness, is due to the discovery of Mlle. Cléret. 2d. Whether results as unfortunate as that of her own case have not previously been observed. 3d. Whether the improvements that the committee believes to have resulted from the use of ether, deserve entire confidence; and whether other empirical means have not produced equal or even superior effects. Finally, how should pretended wonderful remedies generally be regarded. Let us examine each of these questions.

The employment of ether in the treatment of deafness is no new thing, as is well known. In the beginning of this century,

* Mlle. Cléret has become insane, and has been placed in the hospital of Charenton.—*Journal des Connais. Med.*, May 30, 1860.

Itard often made use of this agent, and abandoned it afterwards as unreliable and even dangerous. About the same time J. H. Curtis, in England, employed the same remedy for a time, and subsequently renounced it. Since then Kramer, in Berlin, and French physicians in general who have had occasion to treat deafness, have passed the vapor of ether through the Eustachian tube into the middle ear. But it must be confessed that if we have no terrible disasters to regret, such as that of Mlle. Cléret, we have yet no brilliant success to record. Ether acts as a powerful and painful stimulant when brought into contact with the mucous membranes, especially those of the eye, the ear and the nose, which derive from the fifth pair nervous filaments of exquisite sensibility. The middle ear in particular, which owes its extreme delicacy to the tympanic plexus, suffers intense pain on the application of any stimulant. A very small quantity of vaporized ether penetrating the external or middle ear, produces a sensation like that from the application of a red hot iron. It cannot then be considered a harmless agent, especially if its use be repeated. And further, it is well known that the ear is separated from the brain only by a plate of bone thinner than an egg-shell. Nevertheless, if the utility of ether were well established, nothing more would be necessary than to regulate its administration according to the principles which prudence would dictate to every honest and conscientious physician. But that it is not useful, I shall presently show. Nor is ether the only stimulant that has caused grave disorder of the brain, when employed in the treatment of deafness. I have been consulted in the cases of two ladies whose deafness had been treated with electricity, and who had consequently lost their reason. One of them suffered all the miseries of general paralysis, which supervened two years after the commencement of her insanity. In both cases violent pains were felt during the application of the electricity, and in a few weeks mental hallucinations and madness came on. Mlle. Cléret has therefore discovered nothing new on this subject, and moreover, the calamity that has befallen her is not unprecedented.

In glancing over the annals of our art we should carefully

observe that at all times numerous empirics have pretended to effect the cure of deafness, each one with some new remedy, and all with success, if we are to believe their own statements. Thus in the time of Asclepiades, the deaf were made to hear by the employment of the trumpet; Dioscorides met with equal success by burning sulphur in the ear; and Kirscher, guided, as he says, by a Divine inspiration, owed to this his own recovery. We must say then, that if the committee wished to give real value to their report, they should have stated the causes of deafness in the several patients submitted to the ether treatment; and they would perhaps have found many cases in which it was due to the accumulation of ceruminous matter in the ear. This concretion is composed of elements soluble in ether, and thus are some of the cures explained. For ten years I have devoted my attention to the subject of deafness, and I am convinced from many cases that have come under my notice, that ether, arnica, electricity, and especially galvanism, after having partly aroused the hearing, have in the end caused a more profound and irremediable torpor of this delicate sense.

Attentive consideration of the facts that I have stated, may show the truth of the proverb, *Nihil novum sub sole*, and of that other one, *Vulgus vult decipi*; and thus it will always be as long as the earth revolves. But the duty of physicians is to oppose such abuses by endeavoring to throw real light upon diseases.

We all remember a story which afforded such amusement to Grimm and Diderot, and which they have preserved in their literary, historical, and critical correspondence. Let me tell it in a few words: "At the end of the last century the great ladies of Paris were visited by an epidemic of nervous maladies, vapors, convulsions, &c. A soldier of the guards conceived the idea of assuming the guise of a physician, and administering to these rich patients a potion of his own composition; and thus he cured a number of reputed incurable cases. Not long afterwards his fortune had increased so much that he was able to divulge the secret of his drug, which proved to be nothing else than a '*decoction of hay.*' And although (adds Grimm)

he had given such large doses to his patients, he had yet enough hay left for a span of fine horses which drew his coach, while many a regent of the Faculty was forced to go on foot."

II.—*Puerperal Eclampsia, treated by Subcutaneous Injections of Morphia.* By SCANZONI, of Strasburg. (Bull. de Therap.)

THE attention of the medical public has recently been awakened by the statements of M. Wood, and afterwards of Messrs. Hunter and Bahier, with regard to the beneficial effects of subcutaneous injections, and particularly of narcotic injections. I could easily swell the list of successful cases of neuralgia and hyper-æsthesia healed by this plan; but I prefer to relate a case of Eclampsia which sustains the conclusions of Hunter, by showing that we possess in these subcutaneous injections a means of acting upon and controlling irritation of the brain more rapid and more certain than by the administration of the same agents by the stomach.

It will not be disputed that opium and its preparations occupy the first place in the treatment of puerperal eclampsia. For myself, I have been convinced by observation that a species of intoxication from opium conduces more surely to a favorable termination in this disease than the use of all other means recommended against this cruel malady. Unfortunately the administration of opium or morphia is not always an easy task. Coma, or the rapidity with which the spasms succeed each other, oppose the administration of the remedy by the mouth, and enemata are immediately rejected. A mode of overcoming these difficulties is offered by the hypodermic treatment. Numerous experiments with this mode of treatment have satisfied me that if its effects cannot be always said to be permanent, as in the cure of neuralgias, it nevertheless produces in a very short space of time (in some cases

a few minutes suffice), effects which leave no question as to the action of the opium on the brain. The contents of a single syringe half filled with meconate of morphia, and containing about $2\frac{1}{2}$ grammes (125 milligrammes of opium) thrown into the cellular tissue, constantly produce heaviness, drowsiness, headache, pain at the præcordia, a sense of contraction at the œsophagus, and even vomiting, with functional depression of the nerves of the breast (*sein*), and these phenomena may progress, if the dose be stronger, even to somnolence. Considered in connection with facts already known as to the use of the hypodermic treatment in *delirium tremens*, mania, chorea, tetanus, &c., these results induced me to apply this method to the treatment of puerperal eclampsia with the most gratifying results; for, as will be seen in the following case, this plan reduced the number of attacks from three in one hour and three quarters to two in nine hours. This diminution in the frequency of the attacks is the more remarkable since experience has shown that the paroxysms not only become more and more intense in proportion to the advance in the labor, but that as a general rule the interval between them is also shortened. I am far from believing the hypodermic use of morphia to be an infallible panacea against this fearful disease, but it strikes me that the character of the case is such as should engage medical men to try this plan.

CASE.—D***, æt. 21, a strong and robust primipara, was brought into the labor ward at three quarters past seven on the morning of the 8th June, 1859. Labor had commenced during the night, upon which she was seized with violent nervous attacks, during which she lost consciousness. No further information was given as to character or duration of these attacks. The patient remembered nothing which had happened during the night. The whole body, and particularly the legs were œdematous. The tongue bore on its right side marks of numerous bruises from the teeth, the fundus uteri was at the epigastrium, and its consistence firm. The foetal heart-sounds were very appreciable. "The touch" being instituted, the vaginal portion of the neck of the uterus was

found effaced, the orifice dilated to the size of a piece of fifty centîmes, the waters formed with presentation of the head. The urine was very albuminous and offered under the microscope many fibrinous cylinder casts to view. With this knowledge came the probability that the attacks of the preceding night were eclamptic, and this presumption became a certainty at eight o'clock, at which time she was seized by a second well marked attack of eclampsia, which lasted some minutes, after which her senses returned, and she answered questions, though slowly. A third attack came on at 8.45, a fourth at 9.45, a fifth at 11.45, and a sixth, which was the strongest of all, at 5 o'clock. Consciousness was lost after the fourth attack, nor did it return, and the respiration became stertorous. At 10 o'clock v. s. ad. 250 grm. enemata with 25 drops of an opiate; bath for the whole body, and cold irrigations of the head while the body was in the bath. As opium could not be introduced into "the natural ways," a solution of meconate of morphia was injected under the skin at three different times; the first with 25 centigr.; the whole being 75 centigrammes of opium. The labor progressed slowly, the pains being very rare. At 3 o'clock at night the waters broke, the orifice was the size of a three-franc piece, the head high above the superior strait, and the heart-sounds still very distinct. From this time dilatation was more rapid. At 7 the os was as large as a piece of six livres, extensible and dilatable, but the head still high and immovable. Insensibility complete, with profound coma. In this dangerous condition I dreaded, notwithstanding the position of the head and the incomplete dilatation of the os, to put on the forceps. This of course was not very easy, but the extraction presented no difficulty. After a few tractions we extricated a child which breathed feebly, but did not fail shortly after to scream lustily. The afterbirth followed. There was no attack during the delivery. Wine with ten drops of tincture of amber and musk were given hourly, with the effect of rousing her a little, but without awakening consciousness. At 11 o'clock there was a short and feeble paroxysm, after which she became agitated

and tried to escape; towards morning she became more calm. At 9 A. M. she answered when spoken to loudly; during the whole day she remained as if intoxicated. Pulse 128. The musk was discontinued and she was ordered lemonade. Towards evening the belly became a little painful. During the night there were several slight attacks of mania, during which she constantly attempted flight. In the morning she answered questions rationally. Pulse 108. The œdema has diminished, belly painful; there are numerous fine and coarse râles in either lung, with difficult respiration. Warm bath, lemonade, infus. ipecacuanha, with oxymel scillæ and syrup of diacodium. In the evening the patient had fully recovered her intelligence. Pulse 132.

June 11: Passed a quiet night; a table-spoonful of ol. ric. was followed by several large liquid stools. Œdema of the labia majora—belly still painful. Same potion as yesterday with 1.5 grain opium. Cataplasms to the abdomen and fomentations of the genitals with chamomile.

June 12: Quiet night—tranquil sleep. Expectoration free; râles still present. Pulse 120. Warm bath. Omit opium in the potion. Urine slightly albuminous but no cylinders.

June 13: Patient's state good; no œdema; belly indolent. During the night there is involuntary escape of urine which yields to the continued presence of a sound. All medication is suspended, and the patient put on good diet, with a glass of chalybeate water every morning. The 17th, albumen was no longer found, and on the 12th July, the patient left the hospital with her child, with the advice to continue the use of the chalybeate for a considerable time.

McK.

III.—*On the Value of Dilatation of the Inner Orifice of the Uterine Neck as Diagnostic of the time of Accouchement.* (Union Médicale.) By M. HECKER.

It is generally admitted, says Professor Hecker of Marburg, that the possibility of introducing the finger through the os

internum indicates in primipara an imminent labor, and in multiparæ a confinement within a fortnight at farthest. This is true in a great number of cases, but the exceptional cases are by no means few. In order to throw some light on this question, the author examined and compared the record of all labors occurring in the maternity of Marburg from 1838 to 1858, as to the time elapsing from the first recognition of this dilatation to the accouchement. This, it is true, does not yield the maximum of certainty in the result, as it may easily have happened that the opening was not diagnosed on the very day on which it first occurred. It has, however, sufficient certainty to render the result interesting.

Of 2,593 there were 946 in whom the neck was permeable. 723 were multiparæ, and 223 primiparæ.

In the case of the multiparæ, the interval in question was as follows :

In 366 or 50	p. c. from	-	-	0 to 7 days.
In 144 or 19.92	p. c. from	-	-	8 to 14 "
In 76 or 10.51	p. c. from	-	-	15 to 21 "
In 52 or 7.19	p. c. from	-	-	22 to 28 "
In 73 or 9.82	p. c. from	-	-	29 to 56 "
In 12 or 1.94	p. c. from	-	-	57 to 88 "

In the primiparæ, the following exhibits the interval :

In 138 or 61.88	p. c. from	-	-	0 to 7 days.
In 31 or 13.90	p. c. from	-	-	8 to 14 "
In 20 or 8.97	p. c. from	-	-	15 to 21 "
In 25 or 8.97	p. c. from	-	-	22 to 28 "
In 14 or 6.28	p. c. from	-	-	29 to 64 "

These two tables are sufficiently strong to show, that if the opinion generally received be not entirely false, it is, however, subject to such numerous exceptions that this sign can not be of any value, when alone, to fix the term of labor.

McK.

IV.—*On the Curability of some forms of Uterine Polypi.*

By M. VELPEAU. (L'Abeille Médicale, April 9, 1860.)

A WOMAN who had entered La Charité for the treatment of an uterine polypus, having been spontaneously relieved just as an operation was about being performed, M. Velpéau profited by the occasion to relate to his students the history of some cases of cure or spontaneous disappearance of uterine polypi, and to draw from them an useful lesson, which may be summed up in few words: Never to be in a hurry to operate unless the frequency and amount of hæmorrhage or some other serious accident, produced by the presence of the tumor, should render the necessity of operation urgent.

The possibility of spontaneous cure, although it be, doubtless, of rare occurrence, should, however, always be taken into consideration in forming a prognosis and estimating the chances of an operation which is certainly not free from danger.

The history of this patient is as follows: A woman entered "La Charité" on the 8th of last February, in the service of M. Velpéau, in consequence of hæmorrhage, excited and maintained by the presence of an uterine polypus. Upon the first examination, the Professor recognized the existence of a small polyp. of the cervix, having the volume of the little finger, slender at its origin and enlarged at its free extremity, having thus the form of a bell-clapper, about three centimetres long, and six or eight millimetres in diameter at its free end. As the patient, at the time of entering the house, was in the catamenial period, M. V. thought it advisable to wait. It was not long before the hæmorrhage ceased; but febrile accidents supervened, which again caused the delay of the operation. These symptoms having disappeared, the patient was re-examined, and finding a considerable diminution in the volume of the tumor, M. V. judged it better again to defer operating. Two days afterwards, on examination, no trace of the polyp. could be detected; it had entirely disappeared. This is not

the first case of spontaneous cure which M. V. has witnessed—the disappearance of the tumor either by a process of softening or by atrophy in a very short time.

A remarkable case of the former process occurred some years ago under his care. A woman had a polyp. of the size of a walnut, and the day for operation had been fixed when the menses appeared, and postponed it. Upon the cessation of the catamenia, the tumor was found softened and partly drawn within the neck, whereupon it was decided to await the result. In the meantime, the patient was seized with serious febrile symptoms, the prelude to a peritonitis, of which she soon after died. Upon post-mortem examination there were merely sufficient traces of the polyp. to show its point of origin. We have said that uterine polypi sometimes disappear by a true resolution. An example of this was related by M. Velpeau. A female, living in a provincial town, came to Paris at least twenty times in about six years, to consult M. V. with regard to an uterine polyp., which caused her much uneasiness; but M. V. having found, during the first consultations, a disposition of the polyp. to diminish in volume and disappear, thought best to do nothing. Each subsequent examination revealed a progressive diminution of the tumor, of which now not a trace is left. But these are not the only circumstances which counsel reserve in prognosis, and delay when there is no pressing need of operation. We often see the hæmorrhage from these polypi cease, and the danger thus removed, they become a mere inconvenience, which habit soon renders tolerable, and their presence causes no injury to the general health. We here give an example of this kind, with which we will close this article:

A woman living in one of the provinces came to Paris 12 years since to consult M. Velpeau concerning a pyriform polyp., of the size of a horse-chesnut, and which bled constantly. M. V. proposed an operation, at which the woman became affrighted and went home. Some time afterwards the hæmorrhages returning, and her health giving way, she determined to submit to the operation, but the day being fixed for

the operation, she again ran off. Six months after, she again appeared, but disappeared in the same way. Upon each occasion the same condition was recognized. Thus twelve years passed in continual postponements. But without anything having been done for this woman, the hæmorrhages gradually disappeared and finally ceased. She was in Paris about a month ago, when M. Velpeau found the polyp. still present, with the same form and dimensions, but without hæmorrhage, and in a condition perfectly compatible with satisfactory health.

McK.

V.—*Cases of Traumatic Diabetes*. (From *Le Journal des Connaissances Médicales*, 20 March, 1860.)

M. P. FISCHER has published in "L'Union Médicale" a very curious case of diabetes, existing in a man who had sustained a fracture of the skull, in consequence of a fall from a considerable height. M. Fanconneau Dufresne has taken the opportunity afforded by this case to make some remarks upon the pathology of this disease, and the light thrown upon it by the experiments of M. Claude Bernard. The occurrence of diabetic symptoms, as consequences of various cerebral affections, has been already observed. In the present case, however, as no alterations were discovered in those parts, lesions of which are found by experiments to produce the symptoms in question, it seemed reasonable to attribute them to concussion of the brain. Several cases served to support this view. In the Hospital de la Charité, a woman was seen who had become diabetic after a fall upon the nape of the neck. An account has also been published of a quarryman in whom diabetes occurred after a fall upon the head. The disease disappeared as the wound healed. In one of Goolden's patients, diabetes seemed to have been produced by the kick of a horse, which had caused a severe wound of the head; in another, by

a fall from a stair-case nine months before the patient's entrance into the hospital; in this case, the occiput was struck, and there was loss of consciousness. Goolden speaks too of a patient whose diabetes was ascribed to a wound inflicted upon the head, eighteen months before he was seen at the hospital. In the *Gazette Hebdomadaire*, of November 18, 1859, the case of a young man is reported, who, after a fall from the second story of a house, was seized the following night with excessive thirst, which marked the commencement of diabetes. In most of these cases the disease was not persistent. They correspond with the results of M. Bernard's experiments, in which he produced a temporary diabetes, by causing a concussion of the nervous centres, as e. g. by striking a dog's head with a hammer.

We may note here the principal diseases of the nervous system in which the occurrence of *polyuria* and *glucosuria* has been observed. The researches of M. Leudet, of Rouen, on this subject are well known. In a woman, 32 years of age, who was attacked while pregnant with paralysis of the third and fourth pairs of cranial nerves, he soon afterwards observed diabetes, which was of very brief duration. Another woman, 43 years old, who had suffered from hemiplegia and epileptiform attacks, was discovered after two years to be laboring under diabetes and albuminuria. A woman of 80 years, who had become hemiplegic, presented the signs of diabetes 18 months afterwards; the foot of the paralysed side became gangrenous, in consequence of which she died.* A fourth woman, 39 years of age, was seized just after her confinement with paralysis and convulsions; *ten years* afterwards diabetes ensued. Ulrich found sugar in the urine of a man who died of abscesses of the cerebrum and cerebellum communicating with the fourth ventricle. Steinhall relates that, in a case of diabetes which had lasted seven years, an autopsy revealed sanious spots of apoplexy between the thalamus opticus and

* According to Dr. Garrod, gangrene is of frequent occurrence in diabetic persons.

tubercula quadrigemina. Glucosuria has also been remarked in affections of the spinal marrow. M. Becquerel gives an example of this in a case of acute inflammation of the cord, and another in a case of spinal meningitis, with an inflammatory tumor of the pia mater and softening and cysts in the cerebellum. Sibert has seen diabetes result from hæmorrhage into the upper part of the cord. It is known that Bernard, in producing apoplexy by an incision just before the crura cerebri, caused sugar to appear in the urine. MM. Krause and Grœfe have shown that compression of the medulla oblongata produces the same effect. According to M. Moes, it is likewise observed as a consequence of galvanic excitement of the cord. But it is not only in concussions and lesions of the brain and spinal cord that saccharine urine is observed. Goolden gives the history of a girl 13 years old, who suffered intense cephalalgia and neuralgia, and in whom all the signs of diabetes are present. Thompson has seen the urine of a patient become saccharine during an attack of facial neuralgia, and return to the normal state as soon as the neuralgia yielded to blister. And, finally, Bernard cites in his lectures cases of persons whose urine would become saccharine whenever they gave way to anger.

Without pretending to explain all these phenomena, it is yet possible to give an account of some of them. . . . With regard to the mode in which alterations of the spinal cord produce glucosuria, it is known that irritating impressions at the peripheral extremities of the nerves, arriving at the brain, are transmitted to the medulla oblongata, and thence are propagated to the liver by the filaments of the great sympathetic—an example of reflex action. The saccharine secretion of the liver being thus increased, a large consumption of sugar-making matter takes place, intense thirst is created, and the quantity of urine secreted is in proportion to the amount of the patient's drink. In three cases in which no sugar is detected, but where there are simply *polyuria* and *polydipsia*, it may be presumed that the irritation is transmitted directly and entirely to the kidneys, and that the great thirst depends on

the abstraction from the blood of a large quantity of water ; the thirst is an indication of the need the system feels of having the waste repaired.

BIBLIOGRAPHICAL RECORD.

- I. *Notes on Nursing.* By FLORENCE NIGHTINGALE. Appleton & Co. New York. 1860.

It is a common assertion that "good nursing is half the battle," an assertion which the best physicians will be, perhaps, most ready to admit. With the most unflinching faith in the virtue of medicines when properly used, if a choice must be made between the best attendance to the sick, without medicine, and the best medicine without adequate attendance, we should say, give us, by all means, the best attendance. We do not now consider exceptional cases; we only mean to assert what we believe would be, as a general rule, of the greatest advantage to the greatest number of sufferers. Fortunately there is not very often need to draw any lines of distinction; there are few communities in the United States destitute of a physician of at least respectable acquirements, and whenever there is a conscientious woman of good will and good sense, he has the material out of which to make a capital nurse. Now we grant that "nursing is half the battle," and we would have patient and friends properly grateful for the nurse's services; at the same time, we must add, that no nurse can be perfect in her line except under the guidance and tuition of a well informed physician.

The administration of drugs is, by some, thought to cover

the whole field of medical art, but the medical man, himself, does not participate in this popular error. With his knowledge of the laws of health and disease, of hygiene and therapeutics, he understands full well that drugs are not the only *remedies*; that in many cases they are of secondary importance, although undoubtedly useful, and that still in other cases, they are useless, or even worse, pernicious. There is an old English maxim which says that in the treatment of disease, you must starve the rich and feed the poor; in this, two simple remedial measures are indicated of vast importance, and upon them, in many cases, the physician would place his principal reliance, making medicine, properly, secondary.

Now the best nurse must act in accordance with laws of which she is entirely ignorant; but with a well-informed physician for a guide, the faithful discharge of her duty may readily, not only assuage the patient's sufferings, but make the difference between life and death to him. Independent of physic, the doctor may direct ice, or wine, or soup to be administered, at stated times, and under certain conditions, to a patient critically ill, during the long and silent watches of the night; neglect of his directions, even the unintelligent mal-administration of these simple, non-medical remedies, may readily reduce a doubtful case to a hopeless one. Every physician has reminiscences of just such things occurring in his practice; and every physician, therefore, for his own credit and the credit of his profession, and the good of humanity, earnestly desires the co-operation of a good nurse. What makes the good nurse? We have said it: conscientiousness, good sense and good will, are the first elements; experience and endurance are necessary; patience with the sufferer, and obedience to the physician, follow from the first attributes. The worst nurse is a noisy, officious person, who is full of her own conceit, and is anxious to suggest, or direct treatment, instead of modestly and faithfully pursuing the line of duty which is properly hers.

Miss Nightingale, who divided the honors with the commanders-in-chief during the war in the Crimea, has recently

given to the world a little work which she calls *NOTES ON NURSING*, and she writes as one "having authority," as indeed she has, upon this subject. She exposes to the inquirer, *what nursing is and what it is not*, and as there is much in her work of the highest interest to all who are entrusted with the care of the sick, we will call the attention of our medical brethren to the most prominent topics she treats of, with such running commentary upon her views or doctrines as we may think appropriate.

"*What nursing ought to be.*—I use the word nursing for want of a better. It has been limited to signify little more than the administration of medicines and the application of poultices. It ought to signify the proper use of fresh air, light, warmth, cleanliness, quiet, and the proper selection and administration of diet—all at the least expense of vital power to the patient." P. 8.

This is sufficiently comprehensive, and requires for the most part, the *direction* of an accomplished physician. Its different parts are treated of in sections, the first of which is on

Ventilation and Warming.—Miss Nightingale's great primary law, the *first rule of nursing*, is "to keep the air the patient breathes as pure as the external air, without chilling him." This is a great desideratum, and should be observed as nearly as possible. The old-fashioned plan of keeping air and light out of the sick-room is now pretty well abandoned, yet in numerous families, the sick-room is warmed by close stoves, or furnace heat, in winter, with every window closed, and the fire-place shut up by a close-fitting fire-board. Miss Nightingale suggests an open fire, and when admissible, an open window at the same time, without exposing the patient, however, to direct draught. It should be remembered always, that the room must be ventilated, without being chilled. She raps severely the folly of leaving doors open, which often admit effluvia from about the house that are objectionable, while the windows are kept shut, and *pure* air is thus excluded. "I have seen a careful nurse airing her patient's room through the door, near to which were two gas lights (each of which con-

sumes as much air as eleven men) a kitchen, a corridor, the composition of the atmosphere in which consisted of gas, paint, foul air, never changed, full of effluvia, including a current of sewer air from an ill placed sink, ascending in a continual stream by a well stair-case, and discharging themselves constantly into a patient's room." Who of us has not seen something very alike the above description ?

Our authoress spares not those who leave uncovered chamber utensils hid away under the bed ; she will not have such things left in the room, *with contents*, at any time ; they must be *taken out* and washed, and not emptied in a slop-pail, in the room, to save trouble, or appearances. These little matters are very important, and she does well to demand proper attention to them.

In the section on the HEALTH OF HOUSES, under the specifications of, 1. Pure air, 2. Pure water, 3. Efficient drainage, 4. Cleanliness, and 5. Light, she urges well known facts which are familiar enough to all medical men, but which are often sadly ignored by the great public, and practically, but too often by physicians.

Ill-arranged water-closets, sinks, house-drains, and sewers, are duly reprobated. Sometimes

"the sewer is nothing but a laboratory from which epidemic disease and ill-health are being distilled into the house. * * * *
* * * An untrapped sink may at any time spread fever or pyœmia among the inmates of a palace. * * * I have known whole houses and hospitals smell of the sink. I have met just as strong a stream of sewer-air coming up from the back staircase of a grand London house from the sink, as I have ever met with at Scutari ; and I have seen the rooms in that house all ventilated by the open doors, and the passages all *unventilated* by the closed windows, in order that as much of the sewer air as possible might be conducted into and retained in the bed rooms. It is wonderful."

Some of us in Baltimore have just the same experience. Indeed, it is wonderful !

Our authoress treats *infection* and *infectious diseases* without gloves. She thinks it would be no very difficult matter to get

rid of the whole lot of them. She appears to think that all diseases spring from one source, just as Dr. Darwin, in his *Origin of Species*, derives all animated beings, from men and monkeys down to crabs and oysters, from one primordial creature, of type akin to Hamlet's cloud.

"Is it not living in a continual mistake (she says) to look upon diseases as we do now, as separate entities, which must exist like cats and dogs? Instead of looking upon them as conditions, like a dirty and a clean condition, and just as much under our own control; or rather as the reactions of kindly nature, against the conditions in which we have placed ourselves.

"I was brought up both by scientific men and ignorant women distinctly to believe that small-pox, for instance, was a thing of which there was once a first specimen in the world, which went on propagating itself, in a perpetual chain of descent, just as much as there was a first dog, or a first pair of dogs, and that small-pox would not begin itself, any more than a new dog would begin without there having been a parent dog.

"Since then, I have seen with my eyes, and smelt with my nose, small-pox growing up in first specimens, either in close rooms, or in over-crowded wards, where it could not by any possibility have been 'caught,' but must have begun.

"Nay, more, I have seen diseases begin, grow up, and pass into one another. Now, dogs do not pass into cats.

"I have seen, for instance, with a little over-crowding, continued fever grow up; and with a little more, typhoid fever; and with a little more, typhus; and all in the same ward or hut.

"Would it not be far better, truer, and more practical, if we looked upon disease in this light?

"For diseases, as all experience shows, are adjectives, not noun-substantives." (Pp. 32-33.)

This is carrying the unity of disease to a length not generally dreamed of in medical philosophy. We fear this excellent lady has not drank deeply enough at the fountain of science, or she would not think to settle in a few sentences the great controversies of ages. We admit, to the fullest extent, the potentiality of foul air to make some simple diseases infectious, and to make infection fatal; but how can she prove her assertion that small-pox, for example, was not "caught" just where she says it could not have been by any possibility? Is there

any bedding, clothing, paper, military or hospital material, that has not passed through some unknown hands before coming under the observation of patient, nurse, or surgeon? Now, every medical man knows that this most eminently contagious disease may be carried about by as fine a vehicle as a gossamer thread.

We do not say that Miss Nightingale is certainly wrong; we only say that, considering the minute and unsuspected channels by which such a disease as small-pox may pass, her assertion, positive as it is, must remain, as the Scotch verdict says, "unproven." It is a notable fact that ladies, even those of the keenest powers of observation, habitually let their opinions pass into convictions; they have no philosophic doubts, like men; but what they think to be true they assert to be true, absolutely and unconditionally.

Some of our readers may blame us for conceding that her opinion, though unsupported, may possibly be correct. Without arguing the subject, we give them the following note from Wilson, on *Diseases of the Skin*, as food for reflection:

"Dr. Litchenstein, in a paper entitled, 'On the sources from which matter preservative against the small-pox has been derived,' in Hufeland's Journal for 1841, remarks that limpid lymph taken from the pustules produced by tartarized antimony, and inoculated in a person who has not been vaccinated, produces vesicles which cannot be distinguished from those of vaccinia. These vesicles appear to be equally protective against small-pox with the cow-pox, and the matter may be transmitted from person to person in the same manner. The author of the paper has inoculated and re-inoculated thirty-one persons with the matter procured from this source; and these persons were protected during an epidemic of small-pox, although placed in association with patients affected with that disease."

Under the section on NOISE, Miss Nightingale cautions nurses (be it remembered always, the nurse is the attendant, whether she be the lady of the house, or other lady, or a hired person,) against *whispering* in the sick room. Every patient and every physician will concur in this caution, for whispering, particularly in prolonged conversation, is often more annoying and

more injurious to the sick than speaking what is necessary to be said in the ordinary or somewhat subdued tones of the voice. One may occasionally read aloud to the sick, slowly and distinctly, not hurriedly.

Under the sections *TAKING FOOD*, and *WHAT FOOD?* she urges a middle course between repletion from too frequent feeding, and fasting from too long intervals between the times of giving nourishment. Attention to this matter requires the highest exercise of judgment, and the intelligent nurse should always look to it. Life or death, here, often depends on the discretion of the nurse.

Our authoress commends, for sick and well, home-made brown bread, in preference to baker's bread. In this we entirely concur with her, always provided bread be *well* made at home. We know some families of ample means who *never* have good home-made wheat bread. She commends beef tea, or other juice of meat, as grateful nourishment for the very sick, while she thinks little of jellies, notwithstanding the amount of nitrogen present in gelatine.

"The reason why jelly should be innutritious, and beef tea nutritious to the sick, is a secret yet undiscovered, but it clearly shows that careful observation of the sick is the only clue to the best dietary." (Page 73.)

We will quote a paragraph from one of her notes commending "the cup that cheers yet not inebriates." It is needless to say how very grateful a moderate allowance of good tea is to the sick. This passage is for the well:

"The almost universal testimony of English men and women who have undergone great fatigue, such as riding long journey without stopping, or setting up for several nights in succession is that they could do it best upon an occasional cup of tea,—and nothing else."

We endorse the statement cordially. We are much tempted to digress here into a comparison between the sustaining influences of tea and diluted alcohol in its various forms. We will spare the reader though, except going so far as to remark that

the French and other trappers, who go almost to the poles for furs, take a good supply of tea, which is their greatest solace, while they reject *all* strong drink, as enervating and in every sense pernicious. This is no theory for moral effect, but practical wisdom derived from well-confirmed experience.

That the apartments, beds, bedding, clothes and persons of the sick should be kept as clean as possible, is a fact that calls for no commentary. Without access to general baths, the person may be kept clean by the use of a rough towel, wet with hot water dashed with spirit. We always consent to the *external* use of whisky, and approve of this direction.

Rooms should be dusted with a damp cloth; thus the dust will be taken up, and carried off. It does no good merely to drive it about, with an ordinary duster, from one point to another.

The sick-room should not be kept dark in day-time, except for special reasons. As a general rule, light is both agreeable and salutary.

Miss Nightingale asserts that the English nurses (professional) are inferior to those of Europe generally. So we should suppose. Some years ago, a "Lady Volunteer," one of those who followed Miss N. to the scene of operations during the late war with Russia, published her experience in an exceedingly interesting book, (*Eastern Hospitals and English Nurses*: London, Hurst & Blachete, 1857,) in which she records most feelingly the derelictions of the hired nurses, their infidelity to their duties, and the troubles and mortifications they brought upon the excellent ladies who had to overlook their operations. In this country, the principal fault of our nurses is want of intelligence and training.

Our authoress wishes to give them proper elevation. In this we assuredly agree with her. They are the constant associates of the medical man; they should be his faithful allies and coadjutors. The more intelligent and conscientious they are, the better for him and his patients. Those who are least meritorious in these respects, are most apt to be the physician's critics, instead of being his faithful assistants, and

such as they effect incalculable mischief to all parties. For all interests, then, the physician should do his part in elevating the character of nurses, so as to render them truly fit for their arduous and responsible duties.

As Miss Nightingale has entered freely upon the field of general hygiene, we shall take occasion here to discuss the points of transcendent importance.

Imprimus.—We would wish to see good drinking water distributed at eligible and convenient localities, throughout all cities, at public expense. It would be a great matter to have a public hydrant, *pro bono publico*, at every street corner. The pumps that are now in use are but a very indifferent substitute for these public fountains, for the water from them is loaded with organic impurities, which are exceedingly detrimental to health at all times; but which, under some circumstances, act as direct and fatal poisons. If any one doubts this, we would refer him to reports made by Dr. Snow to the Epidemiological Society of London. During a cholera epidemic, seventy-three persons died in two days in the vicinity of a certain pump. It was afterwards ascertained that the water from this pump was contaminated by a neighboring cess-pool. This is but an instance out of many.

The evil influences of impure water were not confined to cholera times. A medical reporter tells us that Dr. Snow, "in previous papers, had been able to show the great influence of this supply of impure water on the prevalence and mortality of cholera. He was now able to show, from tables compiled and calculated from the returns of the registrar-general, that the nature of the water-supply had a notable effect on the mortality in times when cholera was not present. That part of London which is situated in the county of Surry is supplied by two water companies. Formerly, the water of both of these companies was polluted with the sewerage of London; and the mortality of the districts they supplied was greatly above that of the rest of the metropolis. One of these companies changed its source of supply to Thames Dilton in the beginning of 1852, and immediately the relative predominance

of the mortality of the districts it supplied was diminished. In July, 1855, the other company also changed its source of supply to near Hampton, and in that very quarter the mortality of that part of London fell, for the first time, below that of the rest of the metropolis, and has remained so ever since. He had not the means of ascertaining all the diseases which were diminished by the improvement in the water-supply; but he found, from the weekly returns of the registrar-general, that the mortality of diarrhœa and typhus was greatly diminished."

This is surely enough to show what advantage would result to the health of our cities, if pure water was supplied gratuitously to the thousands of families who now use pump-water from necessity. No streams running under cities can furnish pure drinking water. The sources of pollution are numerous and well known.

Secundo.—Our estimable authoress urges the removal of all offensive *dejecta* from the sick-room at the earliest moment possible. Yes, let them be removed, and, if practicable, let all such material be carried, not to a receptacle in or about the house, but beyond the confines of the city itself. Would it not be a great desideratum to have all effete, decaying and putrescent matter carried away daily, as garbage is at present? We have seen this reduced to practice. In the city of Mexico, about sunset every evening, a long procession of carts, or hogsheads on cart-wheels, may be seen traversing the streets. One of these carts stops at every door, and a servant brings out one or more covered buckets, which the driver empties into the hogshead. This bears upon it an apparently facetious label,—*esencies*. These nameless, but odorous, *essences* are then taken off, to be used as manure. We believe this is the very best plan for getting them out of the way.

The whole air of cities is polluted by the emanations from the sinks. A London physician, Dr. Barber, exposed various animals, from dogs to mice, to the foul air from them, and the effect was more or less fatal, according to the concentration of

the effluvia. Indeed, the accidents that happen to night men are a sufficient proof of this fact, without the aid of experiment.

We transcribe the following passages, deduced from Dr. Barber's experiments :

“In the first place, it cannot be doubted that cess-pool emanations are, when steadily inhaled, poisonous. The dogs, subjected to the cess-pool air, were all more or less affected. The symptoms were those of intestinal derangement, followed by prostration, heat of the surface of the body, distaste for food, and those general signs which mark the milder forms of continued fever, common to the dirty and ill ventilated homes of the lower classes of men.”

And elsewhere :

“The symptoms arising from sulphuretted hydrogen are sub-marked, and may be considered specific. *Vomiting and diarrhœa are the first and most prominent symptoms.* The latter is painful; the vomiting is difficult and exhausting, and eventually there is insensibility and entire prostration.”

Now sulphuretted hydrogen always abounds in these emanations.

We have italicised a sentence which shows that the first and most prominent symptoms from the inhalation of sulphuretted hydrogen are precisely the same which introduce the diseases of the bowels, so common among us every summer. Children, of course, are most readily impressed, and we have long been convinced that the prevalence and fatality of cholera infantum, and kindred summer affections among them in our cities, are more attributable to the *foul air respired, than to any aliments taken into the stomach.*

Now, if adequate measures were adopted: 1st, to give pure drinking water to all classes of citizens; (that is, to the well-to-do, as now, at their own private expense, and to the needy at the public expense;) and 2d, to render the air sweet and pure by removing the principal sources of its pollution; we venture to assert that our cities generally, but especially such as Baltimore, with its great natural advantages, would be

blessed with a degree of health unsurpassed, if rivalled, in the whole world. May the good time come speedily.

We have diverged somewhat from the text of our authoress; the importance of the subject, which led us off, must be our apology.

As a finale, we would say of her work, it is full of interesting and useful matter, and the only fault we have to find with it is, that the authoress is rather too much addicted to positive dogmatism. Deeper draughts from the fountain of science would have made her, at times, more cautious and reserved in her expressions, as all the greatest and best men in our profession are, when treating of matters that are essentially obscure and doubtful.

R. McS.

Baltimore.

II.—*Therapeutics and Materia Medica*.—A Systematic Treatise on the Action and Uses of Medicinal Agents. By ALFRED STILLE, M. D.

WE have merely time this month to acknowledge the receipt of Doctor Stillé's able production, promising in a future number a more extended and critical notice. To say that it is in every way worthy of the author, would be high praise, but we feel assured that our readers will join us in considering it a valuable treatise on the most difficult and intricate science connected with medicine. We confess we are proud that it is an American work—especially when we consider that now the two best treatises on Therapeutics in the English language (Dr. Wood's and Dr. Stillé's) have come from our own country. We know no English works to compare to them. On *Materia Medica* proper, of course, Pareira stands pre-eminent, but even then we have "Wood & Bache," as complete a work as could be made. We hail Dr. Stillé's book with great pleasure, because *Therapeutics*, and not *Materia Medica*,

is the prominent subject he dwells upon; and may we not hope that it may have its influence in inducing the Professors of this important branch of medical teaching to follow his example, and instead of wearying the attention and burdening the memories of their classes with the minute details in regard to the form, shape, &c., of the dry leaves, flowers and roots, and then, merely in passing, say that they have diuretic or purgative properties—instead of this, to show the medicine, and take up the hour in the more acceptable and more useful duty of showing its application as a therapeutic agent; or what would be better still, if the chairs could be sub-divided and we had a lecturer on *Materia Medica*, and another on *Therapeutics*.

With the many excellent works on *Materia Medica*, Dr. Stillé has wisely chosen, as he himself states in his preface, to make scientific unity and precision subordinate to practical utility. Well did Claude Bernard say that *Therapeutics* was the branch of medicine towards which all the others converge. Even in this short notice, we cannot omit to mention how agreeably we have been impressed with the fact that Dr. Stillé is so much less dogmatic in his teaching on this branch than authors generally are. He every where recognizes the principle, "*Medicus curat, natura sanat morbos*," "that medicine is only the handmaid of nature, the really active and efficient agent in the restoration of health. Medicine can do no more than remove the impediments from Nature's path, support her when faint, restrain her when violent, and guide her when she is inclined to err. But the vital powers and functions of the organism have an inherent tendency to return to their normal condition when deranged by any cause, and to remove or repair the alterations of structure which may have attended that derangement." He further states, that "the highest achievement of art is to sustain nature, that she may have time, if possible, to perfect her work."

Not many years since, we heard an old Professor say, "when nature came in at the door, we must throw her out the window."

We are glad to find Dr. Stillé bold in the advocacy of the doctrine that Therapeutics does not differ from the other natural sciences in "being founded," as Humboldt expressed it, "upon a rational empiricism; that is to say, upon the totality of facts recorded by science, and subjected to the mental processes of comparison and combination." This view, so manfully and, to us, successfully maintained by Bartlett in his "Philosophy of Medicine," has not received, by any means, general acceptance by prominent writers on the treatment of disease. These often have said that the "numerical method" was well enough in pathology but was not applicable to therapeutics. Is not this fallacious mode of reasoning one, at least, of the causes of the uncertainties of our art? We have had, even of late years, as in Dr. Headland's work, too much of theories, often brilliant and attractive, but not founded upon the only true basis—that of experience. Dr. Stillé observes with force: "If experience has any value in Therapeutics, it is because similar pathological conditions are constantly re-producing themselves, and are cured by the same methods of treatment." And if morbid conditions obey a law in their mode of development and decline, the medicines which more or less uniformly modify those processes must assuredly do so in virtue of an inherent law. If, therefore, the pathological conditions can be ascertained, it can also be determined under what circumstances they are influenced by medicines so as to terminate in health. To deny this would be also to deny that the effects of a medicine in disease can be anticipated at all, or that Therapeutics is any thing but guess-work. And, still further, it is not by the application of numerical analysis to the influence of particular medicines upon diseases regarded only as units that we exhaust its usefulness. For the greater number of diseases, nearly all, indeed, consist of a few definite changes in the blood or solids, producing symptoms which differ with the organ affected, &c."

We must all acknowledge, that to ascertain laws in Therapeutics is no easy matter; for, in addition to the disease, we have the individual affected, to modify the symptoms, and conse-

quently the treatment. Yet all these conditions must be considered, and then, by putting the results together and classifying them, is the only way of approximating to the truth. Proceeding on such sound philosophical principles, Dr. Stillé analyzes the statements made by others and gives them their real worth.

We ought to add, that the style and language of our author is clear and chaste, and the whole treatise shows him to be an accomplished scholar, familiar with ancient, and 'au courant' with modern medicine.

EDITORIAL AND MISCELLANEOUS.

THE CLIMATE OF THE GLADES,

2,800 FEET ABOVE THE TIDES OF THE CHESAPEAKE.

THE yearly increasing facilities of travel are producing changes in the customs of our urban populations. The season for travel has now arrived. Since early spring, one of the first questions between friends and acquaintances, when meeting, has been, "where are you going this summer?" and now the time has come when the decision must be final. Excursions from the city, for the benefit of health, are now as general and necessary a habit for all families who can afford it as reading the newspaper, or eating a breakfast. The choice of a place should not be guided by prejudice or taste alone, but with a due regard to the principles of climatic changes, the nature of the bath and medicinal waters, and the adaptation of the locality to the temperament and condition of the individual. These things being properly taken into consideration,

will prevent, as far as possible, an erroneous application of time and money. Hitherto, the more obvious and tangible properties of mineral waters have attracted attention, and directed study. As a general thing, the stronger the waters could be made to appear, on analysis, the more they were sought after. But experience and knowledge have corrected this fallacy. The adage, "what is potent for good may be potent for evil," has been often verified with regret in the after effects of an excessive imbibition, during a few weeks' tour amongst our *aqueous sanatoria*.

The curious may find much to interest them concerning climatology and medical geography in the learned work of Dr. Von Mühry, who has discussed the subject from Finland to Patagonia in its breadth and importance; but more is still to be learned from the means of rapid travelling which science has lately placed at our command, and which the progress of the age is daily and fully developing. All aerial districts give us an homogeneousness of atmosphere truly surprising. Chemists have determined its constitution to be of oxygen 23.015, and nitrogen 76.990 parts by weight, together with three to six thousandths of carbonic acid, and a proportion of aqueous vapor varying with the location and meteorological circumstances. So long as this constitution remains undisturbed, atmospheric air is wholesome and invigorating to animated nature. The significance of the often used expression, "pure or impure air," although strictly local, has such a practical application to summer excursionists that an interest is at once attached to the examination of the subject in detail. First—What do we mean by *impure air*? From what do we fly? and whither?

The salubrity of the air of towns differs from that of the country as 22 differs from 34. That is, where an individual has a chance of attaining the age of 22 years in the air of a town, he would have an equal chance of living to the age of 34 years in the air of a salubrious country residence. The same relative difference exists in vegetable life. The corollary, therefore, follows that out-of-town residence during the summer months, when the city air assumes its most noxious phase,

is, of itself, a means of prolonging life; and in proportion to the adaptation of the climate (air) sought, to the healthfulness of the individual, this prolongation of life must be greater or less. The impurities thus far found in the air of towns are, the absence of ozone, an undue proportion of carbonic acid, the presence of sulphur compounds, and an excessive amount of organic matters. In some cities, especially those west of the Alleghanies, this increase is as 9 to 22. This difference between the tainted atmosphere of town and the purer air of country localities is obviously a cause of disease upon a large scale, as the increased mortality proves.

It is not necessary, or even expedient in all cases, to seek the aid of medicinal waters. The more radical the change of air can be made during the brief period the business man of the Atlantic or Western cities can devote to this purpose, the greater will be the results. It is gaining health, as it were, by steam. When Aladdin leaves his marble palace, or his brown-stone front, it is with the fair understanding that the thing must be done quickly if at all, and the sooner done the better. Fortunately there is convenient to all who will avail themselves of its benefits, a *table-land* on the top of the Alleghany mountains, traversed nearly twenty miles by the Baltimore and Ohio Railroad, where the denizens of the Eastern and Western cities may meet in pleasant intercourse, inhaling health with every breath, on the terms suggested by Asclepiades himself, *cito, tuto, et jucunde*—quickly, safely, and agreeably. The peculiarity of the air at this point should invite further study and, as a beginning towards this end, we, as medical men, propose to give the results of our observations and experience during a recent professional visit to the locality.

On the 17th of May, 1860, the cars left Baltimore at 8.30 A. M., and brought us in nine hours to Piedmont, situated at an altitude of 728 feet above the tides of the Chesapeake. From this point upon an up grade of seventeen miles, rising 116 feet to the mile, we ascended to Altamont, a height of 2,620 feet above tide-water. From green and fertile valleys below, we glided to the mountain top, to a region of lofty

looks as bare of verdure as in January; a fact which in itself will give some idea of the change of air or climatic difference. A feeling of buoyancy, with slight headache, is experienced, together with increased strength, innervation and cheerfulness. The voice sounds differently. Digestion is strengthened, sleep improved, the circulation is excited, the skin becomes more ruddy, and the cutaneous perspiration is increased, notwithstanding the diminished temperature, and even when the secretions are not stimulated by exercise.

“Climate,” says Dr. Forry, “constitutes the aggregate of all the external physical circumstances appertaining to each locality in its relation to organic nature.” Authors upon the subject of mountain climates, for the sake of system and comparison, have divided the earth’s surface into three vertical regions. 1. The lowest, or hot regions, would extend in our latitude to the height of 1,000 feet, having a mean summer temperature from 72° to 81° F. 2. The middle, or temperate region, extending from 1,000 feet to 3,000 or 3,500 feet, with a temperature from 54° to 72° F. 3. The superior or cold region extending above this height and never resorted to as a summer residence. The Glades (as this mountain table land is called), are pleasantly situated in the midst of this temperate region in an altitude of 2,800 or 3,000 feet. This height being now traversed by a first class railroad, gives our physicians and inhabitants of the lowlands on either side, advantages which have only to be known in order to be availed of and appreciated.

The village of Oakland, where the Glades Hotel gives the attraction of excellent accommodation, is in round numbers 100 feet lower than Altamont or Cranberry summit—a difference not in itself likely to affect the climate very materially, although we were assured of the fact, that the dew is often frozen at Oakland, when it remains liquid at Cranberry Summit. The advantage of complete and accurate thermometric and barometric data, together with the dew-point, is wanting to compare this climate with others in detail. From observations made by Mr. John Dailey, proprietor of the Glades

Hotel, we will assume that the range of the thermometer is 13° F. lower than the average of Baltimore during the summer months.

The average barometric height for this altitude has been carefully calculated from this data, by Professor John H. Alexander, LL. D., of Baltimore, and although the result thus obtained (and the best at present within our reach) are hereafter to be verified or corrected by more accurate observations and instruments, yet they will not be found far from absolute accuracy. The following table has been constructed to show this difference in figures. It was originally proposed to extend the comparison to many other points, as Norfolk, Washington, Richmond, Philadelphia, Memphis, St. Louis and Natchez, but the meteorological observations of the Smithsonian Institute being now in the process of printing and not therefore accessible, have prevented the extension of the table for the present:

	Av. height of thermom. for the summer mo's of June, July and Au- gust.	Av. height of barometer for the summer mo's of June, July and Au- gust.	Dew point for the summer mo's of June, July and Au- gust.
Oakland, above tide water, 2,500 feet.....	58° F. (?)	27° ?	
Cincinnati, 453 feet ab'e the ocean.	73° 7'	29° 446	
Baltimore.....	71° 6'	30°	60° 8'
New Orleans.....	83° 38'	30° 240	

Assuming then, as a well established fact, that change of climate does exert an influence upon morbid conditions, the next subject which naturally presents itself for consideration would be the selection of that which is most suitable for any particular case. As man uses far more air than water, it is a mistake to resort to "aqueous medicines" without some positive knowledge concerning the definite relationship between the remedy and the disease. Those physicians who have frequented our watering places are struck with the popular ignorance upon this subject and its injurious results. There are proper orographic morbid states, endemic on all mountains it is true, but they are not to be taken into account when treating of the physiological changes wrought by moderate eleva-

tions upon transient summer visitors from cities or lowlands. To particularize the conditions or diseases, for the relief of which this elevation could be confidently recommended, is somewhat difficult. All our readers will bear testimony to the truth of the following quotation from Dr. James Johnson :

“There is,” says this author, “a condition or state of body and mind, intermediate between that of sickness and health, but much nearer the former than the latter, to which I am unable to give a satisfactory name. It is daily and hourly felt by tens of thousands in this metropolis, and throughout the empire; but I do not know that it has ever been described. It is not curable by physic, though I apprehend that it makes much work for the doctors ultimately, if not for the undertakers. It is that *wear and tear* of the living machine, mental and corporeal, which results from over-strenuous labor or exertion of the intellectual faculties, rather than of the corporeal powers, conducted in anxiety of mind and in bad air. It bears some analogy to the state of a ship which, though still sea-worthy, exhibits the effects of a tempestuous voyage, and indicates the propriety of re-caulking the seams and overhauling the rigging. It might be compared to the condition of the wheels of a carriage, when the tires begin to moderate their close embrace of the wood-work and require turning. Lastly, it bears no very remote similitude to the strings of a harp, when they get relaxed by a long series of vibrations, and demand bracing up.”

This he calls the *wear-and-tear complaint*, and although now known under other names, still the description is so graphic that the length of the quotation will be pardoned. For this condition at present we would be content to recommend mountain climates. The number suffering is more than equal to the highland accommodations. The emotional effect produced upon the individual in the presence of mountains, combined with the radical change of air, is the proper antidote to the peculiar dyscrasia described by Dr. Johnson.

In conclusion, we would suggest the propriety of *recommending* the medicinal waters proper for the benefit of diseased conditions only. Association with the really sick does not benefit those who only leave home to improve an exhausted

and debilitated body and mind. On the contrary, the tendencies which they excite are prejudicial. The crowding together of every form of disease with moderate health, is a false philosophy to improve the latter, and the sooner it is recognized and acted on the better. The visible effect of mountain air upon the merely languid is as rapid as the circulation of the blood. The chest is expanded, the expression brightens, color returns, strength revives, hunger, health and happiness succeed each other in quick succession, and the mind brightens as the body glows with invigorated energy. These feelings we can vouch for from personal experience. The Glades present attractions also to the healthy in facilities for hunting, fishing, driving, and in the house comforts of "mine host." In fine—its singular and great advantages of clime and accessibility require only to be known, in order that it shall take a leading place among the summer resorts of our country.

EDITORIAL CORRESPONDENCE.

MESSRS. EDITORS: In a late number of your Journal you gave insertion to an article on Diphtheria by Dr. J. Du-Val, to whom we willingly give credit for his frankness if not for his learning; but a review of the paper convinces us that it would have been wiser for the author to have suffered it to lie in the "tomb of the Capulets," or, at any rate, for the Editor to have withheld it from the garish day.

It is certainly refreshing to find a man shoulder his opinion as a musket, to do battle against "*System*," and great should be the terror of the evil-doers against whom the piece is levelled. But it seems to us a little reckless for the bearer to venture into the field without ammunition; and thus it seems to us with respect to the unsupported opinion flatly enunciated that, "Diphtheria is a modification of Scarlatina of the *throat*;" nor are we informed in what other modification consists, or what may be the precise or probable nature of the unsystematized but standard disease with which Diphtheria is compared and identified. We are of opinion that thinking men in the profession will hardly admit the assumption of the author on

the grounds that "here and there we find Diphtheria hand in hand with it" (Scarlatina)—that "twenty years ago I witnessed far greater ravages of Scarlatina than I have heard of by Diphtheria"—or that "many diseases have undergone various mutations and modifications." And as far as statistics of mortality are concerned, we are convinced that true hearted physicians demand a more precise as well as more Christian record of the fatality of a disease among human beings than the computation that "on one farm the owner lost of servants, in the course of a period of forty-eight hours, four thousand dollars' worth."

In analyzing the treatment we are free to confess our want of astonishment at the affirmation of the author's "having retired from the active practice of medicine for several years," while we recognize the goodness of his heart in the avowed avoidance of "merely medical style," and the writing out "in plain terms, for the sake of families and the public at large," of the No. 1 sumac wash, and the No. 2 chlorinated soda wash, as well as of the family tincture of ipecac. and the poultice with or without tobacco. In spite, however, of the remark "that the most skillful observers are unable to trace such diagnostics as can be safely relied on for practical purposes," our author "concludes his communication by giving a brief account of the last case he had of this disease" (Diphtheria), the diagnosis of which we forbear to defend, although we congratulate him upon the happy termination of his case. Nevertheless, as may be supposed from what has preceded, we profess the creed that the diagnosis of Diphtheria, whether positive or differential, can be made and is habitually established "by the most skillful observers"; and that there is almost unanimity among the profession in respect of the rational and empirically successful treatment suggested by the most widely accepted theory of the disease in question. In evidence of what we state, we refer to the very numerous and highly valuable papers which, during the past year, have appeared in the columns of the "Lancet," the "Medical Times and Gazette," the "Gazette Médicale" of Paris, the "American Journal of Medical Sciences," and not least of all, the "Pacific Medical and Surgical Journal," in the last of which we find a most excellent summary of our knowledge upon this subject by Dr. David Wooster. From this able article we borrow a few conclusions, to shew that some definite ideas concerning Diphtheria and its treatment have been formulized by one of many "skillful observers."

"1. Diphtheria is a specific, new zymotic disease."

"2. It is distinguished from Scarlatina, by the absence of eruption; from gangrenous sore throat, by the absence of ulceration and sloughing; from croup, by the aplastic nature of the exudation."

"3. Diphtheria may properly be divided into two varieties; the mild and the severe."

"4. Its diagnostic sign is an aplastic membranous exudation."

"5. The treatment is tonic, antiseptic, stimulant and nutritious."

"Conclusion.—Diphtheria is the most *certainly* fatal epidemic that ever visited our race; but it is not, *de naturâ suâ*, incurable"

In taking leave of the paper of Dr. Du-Val, we beg to be understood as objecting to it on scientific grounds only, and as leaving out of the discussion all question as to the success which has attended the professional efforts of that gentleman.

EDITORS' P. S.—This number is entirely composed of *original matter*, thanks to our valued collaborators. With such support we can hope even for better things.

It is due to Dr. FORWOOD to say that his review of Darwin's work has been in our possession for four months, and hence was not suggested by the repeated notices which have appeared in the journals on that subject.

Also—There was an error in the title of Dr. BURWELL's Essay published in our last issue. It should have read, an *Essay on Vaccination*.

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ART. I. *Clinical Lecture on Over-distention of the Bladder.*

By Professor N. R. SMITH, at the Baltimore Infirmary,
February 9, 1860.

GENTLEMEN: Next to fractures, the diseases less understood as a class, and therefore most badly managed, are those of the bladder. The patient before you, Geo. Springer, aged 34, a man of indifferent, or at times of dissipated habits, entered this infirmary on the 9th inst., yesterday. He is a gardener, and says he has been sick two years. It is scarcely possible to find out from him the first cause of his present difficulty. Perhaps his irregular habits were the foundation of it; because, excepting his local suffering, he appears to be of sufficiently good constitution. It is highly probable that at some one time, or it may have been frequently, when intoxicated he allowed his bladder to become distended and to remain so for a long time. This may have induced an acute cystitis, which has now become chronic, and is attended with impaired innervation or partial paralysis of that organ. In the absence of facts, this theory is admissible, since it accounts for the symptoms he describes in the history of his disease. There is no impediment in the urethra: You will observe I can introduce with

ease a large sized catheter, No. 12. This gives a great facility in his treatment; indeed, without it all other means would be of doubtful result. The bladder is incapable of completely emptying itself, and is therefore subjected to a double cause of irritation. The first is mechanical, from the volume and weight of fluid retained; the second, from the irritating nature of the urine itself, which, when long retained, is constantly undergoing chemical changes. I will direct this catheter to be introduced three times daily so as to completely empty the bladder. Conjoined with this he will take internally an infusion of Buchu and an emulsion of balsam of Copaiba. Generous diet and regular habits of life will improve his general condition, and contribute to a favorable result.*

Retention of urine producing over-distention of the bladder sufficient to cause paralysis is, in so young a man as the one before us, comparatively rare, unassociated with stricture. It is more commonly found in advanced life. Judging from what I have seen, the diagnosis appears not unattended with difficulty. I will endeavor to point out to you the principal sources of deception. If the diagnosis be not correct, the treatment is apt to be false.

Some years ago a physician, in large practice, requested me to visit in consultation a gentleman, aged 70, who had, he thought, a large abdominal tumor. The surface of the tumor was covered with leech-bites and scarifications; it had also been fomented and anointed with only the result of increasing the enlargement. Immediately upon touching the tumor, I detected a distended bladder reaching to the umbilicus. In answer to my questions, I was told that the patient passed urine freely, and that the catheter had been frequently introduced. The consultation resulted in my requesting permission to introduce a longer catheter than had hitherto been used. I selected a large-sized silver catheter, with a deep pelvic curve, on account of the age of the patient, and fully twelve inches in length. It was such an instrument as I show you

* The patient left the Infirmary at the end of a week greatly improved.

now. When the eyes of the catheter reached the bulbous portion of the urethra, there was a discharge of water similar to that the physician had been in the habit of obtaining. But I knew the instrument was only in a pouch formed by a distention of the membranous portion of the urethra. Withdrawing the point slightly, I elevated the whole instrument, *thus*, and felt the point distinctly surmount the point of the prostate and pass through the constricted neck of the bladder. More than two quarts of highly ammoniacal urine escaped, and, of course, the abdominal tumor subsided. The entire length of the catheter was ensheathed in the parts.

This is not an isolated, or even an uncommon instance in my practice. On reflection, the real condition of the case can be easily comprehended. Some urine escaped or dribbled from the bladder, which deceived, both the physician and the patient, and the passage of the catheter was incomplete. More urine was conveyed into the bladder than escaped from it, and hence its distention was gradual. Many persons, and especially old people, will tell you they pass urine frequently, yet easily and well. They think they completely empty the bladder, but by a thorough examination with the catheter, and only by this means, the mistake is discovered. This pathological condition may exist for a long time, without serious inconvenience, but over-distention is always pending, and sooner or later will occur. On the same principle an intestinal hernia, when allowed to protrude, is always liable to become strangulated. A period of imminent danger may at any time arise, because the sources of irritation are constantly present. An elderly gentleman in Virginia had retention of urine, and the physicians in his neighborhood found it impossible to relieve the bladder. I was requested to meet him on board the steamboat when he arrived here. The urine drawn off was most disgustingly foetid, because it had been so long retained. I owed my success in the operation to *this* instrument, and the manipulations already mentioned and hereafter to be noticed. As there was no previous stricture, it was easy enough to reach the membranous portion of the urethra. From this

point the normal anatomical relations had been disturbed. To find room for its immense distention, the bladder had risen out of the pelvis into the abdomen, as the uterus does at the period of quickening. To continue the analogy, the urethra was stretched like the vagina. The membranous portion, being the most yielding, was drawn out, and its calibre diminished. The prostatic portion, being firmer, was bent, and the orifice of the bladder did not present to the point of the instrument. This vicious deviation is always in proportion to the degree of distention present. In this case the orifice of the bladder presented downwards and backwards. Withdrawing the point of the catheter slightly and elevating the whole instrument, (in the manner which I show you now,) at the same time assisting this elevation with the index finger of the left hand in the rectum, the point of entrance was found and passed. It was necessary to repeat this operation in less than four hours. This is of the utmost importance to remember in cases of over-distention of the bladder. As a result of the distention, the kidneys are hindered in their physiological office. The circulating fluids become surcharged with morbid matters which should be eliminated. The skin and the breath have a urinous smell. As soon as the bladder is relieved, the kidneys are prompted to a most vigorous action. There being a large quantity of elementary materials of urine retained in the blood, it is a beautiful and wonderful instance of the response of nature to a necessity.

Fever, attended with thirst, followed; diluents were demanded with avidity, and were administered. The bladder was emptied six times in the first twenty-four hours. The quantity of fluid withdrawn was always large. Its fetor gradually diminished. During the succeeding day, the catheter was introduced four times, and afterwards thrice daily for a period of three weeks.

I have not had much experience with injecting the bladder, as advised by some surgeons. It has been recommended to inject it with solutions of the sulphate of zinc, corrosive chloride of mercury, nitrate of silver, and various vegetable

astringents and alteratives, and you will find many cases recorded showing beneficial results from this practice.

Retention of urine, causing over distention of the bladder, results from various causes. I will only allude to those peculiar to the female from versions of the uterus, parturient difficulties and hysteria. I have met with it in young men after injuries, either about the pelvis or brain; from strictures either spasmodic or permanent; from protracted voluntary retentions, during the course of adynamic fevers, particularly dysentery, and from irritable hæmorrhoids. In older men, it results from incomplete micturition, enlargements of the prostate, and other anatomical accidents.

An elderly gentleman residing on Federal Hill, in this city, had over distention of the bladder, existing a long time before I saw him. It was plain to me that the point of the catheter entered the orifice of the bladder, yet the urine did not flow, and further ingress of the instrument was difficult. When, however, by a careful manipulation it had penetrated this viscus still further, relief was obtained. I have preparations which will demonstrate to you the difficulty in this case. Either the uvula vesicæ, hypertrophied by inflammation, had folded over the viciously presenting orifice as a valve; or the impediment was caused by an enlargement of the third or middle lobe of the prostate. When this latter accident occurs, the orifice of the bladder is so displaced that it can only be overcome by the manipulation I have described to you. From neglect of sufficiently elevating the catheter in this situation, I have seen a false passage made underneath the prostate, and entering the bladder completely behind it. When no permanent stricture exists, you will do well to recollect these two principles. First, all the impediments to the introduction of the catheter are found upon the floor of the urethra. Second, as a consequence of this, endeavor to sweep the upper surface of the urethra with the point of the instrument. To accomplish this, three movements are necessary: First, introduce the catheter parallel to the body of the patient until the point shall have arrived at the bulb of the urethra. Then, secondly,

depress the handle to a right angle with the body. Thirdly, elevate the whole body of the instrument and again depress the handle. In cases of great over-distention, when the prostatic portion of the urethra is bent, and the orifice of the bladder presents abnormally, the index finger must be passed into the rectum in order to straighten the prostatic portion of the urethra, and conduct the point of the instrument safely through, and it will be found of great assistance in the operation. In fact, the difficulties attending the successful employment of the catheter are very great in some cases, and the surgeon is obliged to resort to many expedients before accomplishing his purpose. Knowledge, dexterity and habit are indispensable to the operator, and hasty and forcible manipulations are invariably prejudicial.

ART. II.—*Extracts from the Sanitary Reports of the United States Army.* By R. H. COOLIDGE, M. D., Assistant Surgeon, Fort Riley, Kansas.

I. FATAL CASE OF POISONING.

ON the first day of March, 1858, three *convalescents* in hospital, privates Corcoran, Smith and Hunt of the first cavalry, discovered a bottle of whisky upon the roof of an out-house, of which the three drank and then hid the bottle in another place. This was about 7 A. M. At 12 M., the same parties went for another drink; they found that the bottle had been removed, but discovered it near the place where they had concealed it. The three again drank, and offered some to a nurse, who took a small portion, but refused to take more, on account of its peculiar taste. The three men then went to dinner, soon felt uneasy, and complained to one another of pricking sensations in the limbs, dizziness and nausea. On returning to the ward, Corcoran laid down on a bench and soon vomited.

A few minutes after vomiting, he attempted to walk to his bed, but fell upon the floor, being unable to stand. By this time, both Smith and Hunt were vomiting. The steward was now called, gave emetics of ipecac. to the three and sent for me. On reaching the ward, my attention was directed to Corcoran, who complained of burning in the throat, excessive nausea and cramps in the abdomen. His face was somewhat livid and very anxious; his hands and feet were cold and moist. I asked a few questions respecting the nature of the poison, to which he was replying, when he was seized with violent convulsions, during which the head and neck became much congested. By dashing cold water over the head and chest, and slapping his face and chest with a cold, wet towel, he was revived. I then hastened to the surgery for stimulants; on my return, a second convulsion was beginning, which terminated in death at half-past two o'clock, fifteen minutes after my reaching his bed-side, and two and a half hours after taking the poison.

The condition of Smith and Hunt not being so alarming, I now had time to elicit the particulars above narrated. No clue could be obtained as to the nature of the poison swallowed. By draining the bottle, about a thimbleful of the poisoned liquor was obtained. This tasted only of whisky. It was tested for arsenic, corrosive sublimate and oxalic acid, with negative results. Returning to the ward, I found Smith and Hunt worse. Smith's pulse was 40; respiration slow, and occasionally sighing; he complained of cramps in the abdomen and limbs, of pricking sensations all over the body, and of dimness of vision; his pupils were contracted. An emetic of sulphate of zinc was now given to him, which completely evacuated the stomach. After this, his body and limbs were rubbed with mustard; sinapisms were applied; his hands and feet were placed in hot water; carbonate of ammonia grs. x., camphor grs. v., brandy oz. j., given every fifteen minutes. By testing the few remaining drops of poisoned liquor with iodine, results were obtained which confirmed my suspicions that the poison was *aconite*. In addition to the stimulating

treatment above stated, I now gave twenty minims of tincture of opium every half hour. At one time, Smith's dimness of vision was such that he could not recognize those around him; his pulse sank to 36; he had no feeling in his limbs, nor could he move them, and was, for a few moments, speechless. For seven days he was not able to sit up, without feeling faint and dizzy, and, for the same period, his pulse kept below 50. Stimulants were not wholly discontinued before the seventh day.

Hunt was not so severely affected. After free emesis, stimulants and tincture of opium were administered to him. On the second day, he was attacked with violent pains in the stomach and with vomiting, attended with a peculiar, quick, short, catching and interrupted respiration, and a slow, feeble pulse. He also complained of pricking of the lips and limbs, and of weakness. Stimulants aggravated the pain in the stomach; blisters and purgatives afforded no relief. The pains persisted till the evening of the third day, when they ceased, apparently in consequence of the exhibition of valerian, Hoffman's anodyne, and aromatic spirits of ammonia. Hunt is of a highly nervous temperament, and peculiarly susceptible to pain.

The nurse, who took a very small portion of the poisoned whisky, had pricking sensations, dizziness, muscular debility and nausea. An emetic, a little brandy and two doses of laudanum were all he required.

I think the laudanum was the most potent for good of all the medicinal agents used in these cases.

Nothing has transpired to show by whom the liquor was poisoned, or the quantity taken in the several cases.

II. GUN-SHOT WOUND OF BRACHIAL ARTERY—DEATH IN FORTY-FIVE HOURS.

ON Thursday, April 15th, at 4 P. M., Mr. Coleman was wounded in the left arm by the discharge of a rifle, which he was pulling towards him, muzzle foremost, from the bottom of

a canoe. The accident occurred on the bank of the Republican, a mile or more from Fort Riley. Nearly an hour passed between the receipt of the wound and my arrival. The man had bled most profusely; the blood on his clothes and upon the ground was florid. I could not ascertain whether it had flowed *per saltem* or not; no effort had been made to check the bleeding; his shirt sleeve had neither been opened nor rolled up. The extreme exhaustion forbade much exploration of the wound. Cutting away the sleeve, I saw the wound bleeding in a *slow, continuous stream*; very slight pressure over the wound arrested the flow. A conical sponge was placed in the wound over the brachial vessels, and confined with a bandage. The tourniquet was not required. Stimulants were given; the man was soon conveyed in a wagon to the hospital, where the usual means were assiduously employed to restore warmth and induce reaction. The bandage remaining perfectly clean, I concluded not to disturb it. The wounded arm was cold and blue; there was no pulse at the wrist. On Friday morning, his general condition was somewhat improved; his feet and body were warm; the pulse was more distinct in the *right* wrist, though feeble and somewhat fluttering; he was constantly nauseated and was tossing his feet and right arm. Left arm cold, powerless; the thumb and fingers strongly flexed, and without pulse. I now removed the bandage and compress, first applying a tourniquet to the artery *above* the wound. On removing the compress, there was venous hæmorrhage; the tourniquet was loosened and the bleeding *lessened*; pressure was made *below* the wound, and the hæmorrhage *ceased*. The hand and forearm were now enveloped in cotton-wadding and flannel rollers; the arm was bandaged with moderate firmness: a compress applied over the wound and below it in the course of the artery, and a tourniquet applied, not very firmly, so as to be ready should hæmorrhage occur. In this change of dressings, about one ounce of blood may have been lost, not more; but such was the prostration induced, that I remained several hours by the bed-side, supporting him with carbonate of ammonia, brandy and beef tea. I now

judged that my first impression, that the artery was wounded, was erroneous, and that the hæmorrhage must have been from other sources than the main trunk, and was probably venous; the quantity lost accounting for the florid hue of the blood as first seen by me. I attributed the loss of sensibility and coldness of the arm, as well as absence of the pulse, to injury of the nerves and to the great prostration of the patient. In the entire absence of bleeding, I confined my exertions to supporting my patient. Thus passed Friday morning and night. On Saturday morning, I found the wounded man free from nausea; some beef tea and also green tea had been retained. He complained of headache; but his countenance did not wear the aspect of returning strength. The tourniquet, moderately loose before, was now removed; not a drop of blood was to be seen on the dressings. There was some sensibility about the arm; it was a little warmer than it had been; the shoulder was not swollen, nor was it hot.

I omitted saying that the ball passed through the arm, making its exit through the scapular head of the triceps extensor. The bleeding from this orifice was arrested by a small compress and adhesive strips.

This was the condition at 8 o'clock, though it may be well to add that the warmth about the shoulder was sufficient to induce me to apply to it cloths wet with cold water; finding too that the head was warm, I applied cold water to it also, and diminished the stimuli.

At 12 o'clock, I was sent for. No hæmorrhage; shoulder swollen and somewhat livid; patient unconscious and evidently sinking. Removed all dressings; *skin above* and around the wound in first stage of gangrene; *below* the wound, healthy in appearance, but cold. In a few minutes, the shoulder and one side of the chest became emphysematous, and at 1 o'clock, forty-five hours after the receipt of the wound, death closed the struggle.

Autopsy three hours after death. Mortification confined to the skin in vicinity of wound. The single ball with which the rifle was charged had entered the arm in its superior third,

and at the inner edge of the biceps—ranging upwards, inwards and backwards, it passed out high up through the scapular head of the triceps. In its course it had completely severed and carried away (retraction considered) at least two inches of the brachial artery and its vena comites, leaving only a string of the outer side of their common sheath, that nearest the biceps; the median nerve was wounded, but not severed—as it were, notched; the basilic vein was completely severed. In the upper cardiac extremity of the artery was a clot of much firmness. It seemed to me that the divided artery, having retracted within its sheath, and contracted by its own inherent force, became still further closed by the string-like and continuous portion of the sheath acting somewhat like the string of a purse.

Be that as it may, the man died from exhaustion consequent upon hæmorrhage, superadded to the shock of the injury. The ease with which the hæmorrhage was checked, in the first instance, by an ordinary compress and roller applied with no unusual tightness, and the subsequent entire arrest of bleeding by pressure *below* the wound, deceived me into the belief that the main artery was not wounded, and this, notwithstanding the coldness of the limb and absence of pulse.

In reviewing this case, I am of opinion, that any exploration of the wound, or attempt to tie the brachial artery at any moment *subsequent to my arrival*, must have proved fatal from the venous discharge which would have taken place. Had hæmorrhage returned, or had undue pressure been required for its suppression, I should have tied, or have endeavored to tie, the artery. As it was, I concluded to wait a more favorable condition of the patient, before risking even a slight additional loss of blood. Did I act for the best or not? Death was in no wise the result of the compression; nor was it from any loss of blood after I reached the wounded man. Is it not singular that the cardiac portion of the brachial, so high up as where it rests upon the coraco-brachialis, should have been so firmly and completely closed by its own contraction and a coagulum?

ART. III.—*Ethnological Papers.* By W. S. FORWOOD, M. D.,
of Darlington, Maryland.

[Concluded.]

CHAPTER the fourth elucidates the laws of "Natural Selection." By this term we are to understand the author to mean the "preservation of favorable variations and the rejection of injurious variations." This result is accomplished in many ways, viz: by sexual selection, by the intercrossing of individuals, by isolation, numbers, climate, food, &c.

In this chapter, the author gives a diagram of the growth, divergence, and extinction of species, all of which spread from a few points of origin, becoming gradually more and more widely diffused as they extend through thousands of generations, until a very small number of original types (perhaps only one, as we will hereafter see) have produced through the means of natural selection, the great diversity of genera and species now inhabiting the earth. We regret that we cannot reproduce this diagram here, with the author's lengthy explanations; but as this is impracticable, we will quote the following paragraph, which contains a simile that will give the reader a very correct idea of the author's meaning:

"The affinities of all the beings of the same class have sometimes been represented by a great tree. I believe this simile largely speaks the truth. The green and budding twigs may represent existing species; and those produced during each former year may represent the long succession of extinct species. At each period of growth all the twigs have tried to branch out on all sides, and to overtop and kill the surrounding twigs and branches, in the same manner as species and groups of species have tried to overmaster other species in the great battle of life. The limbs divided into great branches, and these into lesser and lesser branches, were themselves once, when the tree was small, budding twigs; and this connection of the former and present buds by ramifying branches may well represent the classification of all extinct and living species in groups subordinate to groups. Of the many twigs that flourished when the tree was a bush, only two or three, now grown into great

branches, yet survive and bear all the other branches; so with the species which lived during long past geological periods, very few now have living and modified descendants. From the growth of the tree, many a limb and branch has decayed and dropped off: and these lost branches of various sizes may represent those whole orders, families, and genera which have now no living representatives, and which are known to us only from having been found in a fossil state. As we here and there see a thin straggling branch springing from a fork low down in the tree, and which by some chance has been favored and is still alive on its summit, so we occasionally see an animal like the *Ornithorhynchus* or *Lepidosiren*, which in some small degree connects by its affinities two large branches of life, and which has apparently been saved from fatal competition by having inhabited a protected station. As buds give rise by growth to fresh buds, and these, if vigorous, branch out and overtop on all sides many a feebler branch, so by generation I believe it has been with the great tree of life, which fills with its dead and broken branches the crust of the earth, and covers the surface with its ever branching and beautiful ramifications." (Pp. 118, 119.)

This is certainly a very ingenious and happy illustration of the author's theory; but it must be remembered that hypothetical positions are as susceptible of illustration as are self-evident truths.

We next take up chapter the fifth, which treats upon the "Laws of Variation." Their operations are considered in relation to external conditions, use and disuse, acclimatisation, growth, the variability of parts developed in an unusual manner, the analogous variations of species of the same genus, etc. Examples are given on all these points, and others, which show that the author has given great attention and study to the subject. The chapter is concluded with these words:

"Whatever the cause may be of each slight difference in the offspring from their parents—and a cause for each must exist—it is the steady accumulation, through natural selection, of such differences, when beneficial to the individual, that gives rise to all the more important modifications of structure, by which the innumerable beings on the face of this earth are enabled to struggle with each other, and the best adapted to survive." (Pp. 152, 153.)

A very candid statement of the "Difficulties on the Theory," forms the subject of the sixth chapter, together with the author's reply to them. We can only give a very brief *resumé* of them. The objections to the theory are thus stated:

"Firstly. Why, if species have descended from other species by insensibly fine gradations, do we not everywhere see innumerable transitional forms? Why is not all nature in confusion instead of the species being, as we find them, well defined:

"Secondly. Is it possible that an animal having, for instance, the structure and habits of a bat, could have been formed by the modification of some animal of wholly different habits? Can we believe that natural selection could produce, on the one hand, organs of trifling importance, such as the tail of the giraffe, which serves as a fly-flapper, and, on the other hand, organs of such wonderful structure, as the eye, of which we hardly as yet fully understand the inimitable perfection?" (P. 154.)

The third and fourth objections which are here stated, refer to instinct and hybridism, which, as they are discussed in separate chapters, need not be quoted in this place.

The author replies to the first objection, viz: the absence or rarity of transitional varieties, by assuming that natural selection acts solely by the preservation of profitable modifications, and that extinction and natural selection go hand in hand. "Hence, if we look at each species as descended from some other unknown form, both the parent and all the transitional varieties will generally have been exterminated by the very process of formation and perfection of the new form." (P. 155.)

His reasons why we do not discover the innumerable transitional forms imbedded in the crust of the earth will be given in the chapter on the Imperfection of the Geological Record. The author sums up this point by stating his belief that species come to be tolerably well-defined objects, and have not, at any period, presented the inextricable chaos of varying and intermediate links, but have always occupied the same relative positions to each other; the laws of variation operating by a slow and insensible process, so that the points of difference

between parent and offspring can scarcely be distinguished for several generations, and that the ancient forms of our present species have become extinct in consequence of these very transitions, and are now only to be looked for amongst fossil remains. The cause why more of them have not yet been discovered, the author contends, is to be attributed to the very limited extent to which geological research has yet been carried.

The second objection, as to the origin and transitions of organic beings with peculiar habits and structure, is next discussed. The author's answers, however, are too lengthy for us to quote, and would not be intelligible if abbreviated. He asserts that he can show, for instance, that within the same group carnivorous animals exist, having every intermediate grade between truly aquatic and strictly terrestrial habits; and as each exists by a struggle for life, it is clear that each is well adapted in its habits to its place in nature. He gives instances of animals having webbed feet, that do not swim; refers to the amphibia that live in or out of the water indifferently, and speaks of the *Mustela vison* of North America, which, during the summer, dives and preys on fish, and during the long winter, leaves the frozen waters, and preys, like other pole-cats, on mice and land animals. He dwells particularly on the peculiar adaptation of the woodpecker for climbing trees and seizing insects in the chinks of the bark, and tells us that, "on the plains of La Plata, where not a tree grows, there is a woodpecker which, in every essential part of its organization, even in its coloring, in the harsh tone of its voice and undulatory flight, told me plainly of its close blood relationship to our common species; and yet it is a woodpecker which never climbs a tree!"

The following quotation will forcibly show the author's views on the capacity of animals to change their natures. He says,

"In North America the black bear was seen by Hearne swimming for hours with widely open mouth, thus catching, like a whale, insects in the water. Even in so extreme a case as this, if the supply of insects were constant, and if better

adapted competitors did not already exist in the country, I can see no difficulty in a race of bears being rendered, by natural selection, more and more aquatic in their structure and habits, with larger and larger mouths, till a creature was produced as monstrous as a whale!"

This appears to us, with our old-fashioned notions of genera and species, truly a most violent supposition; but this is a fair specimen of the views entertained by the author, and for the illustration of which his present work is put forth.

We must pass over much interesting matter contained in this section for the want of space.

Next in order, chapter the seventh, we come to the subject of "Instinct." The author says that he will not attempt any definition of instinct, but states in a general way that it would be easy to show that several distinct mental actions are commonly embraced by this term; he says that every one understands what is meant, when it is said that instinct impels the cuckoo to migrate and to lay her eggs in other birds' nests. Also, "an action, which we ourselves should require experience to enable us to perform, when performed by an animal, more especially by a very young one, without any experience, and when performed by many individuals in the same way, without their knowing for what purpose it is performed, is usually said to be instinctive." But the author avers that he could show that none of these characters of instinct are universal: he thinks that a little judgment or reason often comes into play, even in animals very low in the scale of nature.

The most extraordinary instinct perhaps in the author's whole series of interesting examples, is that termed by him the *slave-making instinct*; and as the facts of the existence and operation of this instinct are not generally known, we must be permitted to quote largely on this point. Speaking of this instinct, as exhibited by a species of ant, the author says:

"This remarkable instinct was first discovered in the *Formica* (*Polyerges*) *rufescens* by Pierre Huber, a better observer than his celebrated father. This ant is absolutely dependent on its slaves; without their aid the species would certainly be-

some extinct in a single year. The males and fertile females do no work. The workers or sterile females, though most energetic and courageous in capturing slaves, do no other work. They are incapable of making their own nests, or of feeding their own larvæ. When the old nest is found inconvenient, and they have to migrate, it is the slaves which determine the migration, and actually carry their masters in their jaws. So utterly helpless are the masters, that when Huber shut up thirty of them without a slave, but with plenty of food which they like best, and with their larvæ and pupæ to stimulate them to work, they did nothing; they could not even feed themselves, and many perished with hunger. Huber then introduced a single slave, (*F. fusca*), and she silently set to work, fed and saved the survivors; made some cells and tended the larvæ, and put all to rights. What can be more extraordinary than these well-ascertained facts? If we had not known of any other slave-making ant, it would have been hopeless to have speculated how so wonderful an instinct could have been perfected." (P. 195.)

P. Huber likewise discovered the *Formica sanguinea* to be a slave-making ant. Mr. Darwin gives the observations made by himself upon this species:

"I opened fourteen nests of the *F. sanguinea*, and found a few slaves in all. Males and fertile females of the slave species are found only in their own proper communities, and have never been observed in the nests of the *F. sanguinea*. The slaves are black and not above half the size of their red masters, so that the contrast in their appearance is very great. When the nest is slightly disturbed, the slaves occasionally come out, and like their masters are much agitated and defend the nest. When the nest is much disturbed and the larvæ and pupæ are exposed, the slaves work energetically with their masters in carrying them away to a place of safety. Hence, it is clear that the slaves feel quite at home." (P. 196.)

The author watched their nests several hours at various times in the months of June and July, but never saw a slave either leave or enter. Mr. F. Smith also informed him that he had watched the nests for hours during May, June and August, both in Surrey and Hampshire, but had never seen the slaves, though present in large numbers in August, either leave or enter the nest. "Hence, he considers them as strictly house-

hold slaves. The masters, on the other hand, may be constantly seen bringing in materials for the nest, and food of all kinds." The author, however, afterwards observed in the month of July, a few slaves mingled with their masters leaving their nest, and sallying forth in search of food; they marched together some twenty-five yards distant, and ascended a tall Scotch fir tree, probably in quest of aphides or cocci.

We cannot resist the temptation to make another extract from the author's own observations regarding this most wonderful instinct:

"One day I fortunately chanced to witness a migration from one nest to another, and it was a most interesting spectacle to behold the masters carefully carrying, as Huber has described, their slaves in their jaws. Another day my attention was struck by about a score of slave-makers haunting the same spot, and evidently not in search of food; they approached and were vigorously repulsed by an independent community of the slave species (*F. fusca*); sometimes as many as three or four of these ants clinging to the legs of the slave-making *F. sanguinea*. The latter ruthlessly killed their small opponents, and carried their dead bodies as food to their nest, twenty-nine yards distant; but they were prevented from getting any pupæ to rear as slaves. I then dug up a small parcel of the pupæ of *F. fusca* from another nest, and put them down on a bare spot near the place of combat; they were eagerly seized and carried off by the tyrants, who perhaps fancied that, after all, they had been victorious in their late combat." (P. 197.)

The existence of slavery among ants is also noticed by Agassiz and Gould, in their work on the Principles of Zoology, in the following words:

"Some ants make war upon others less powerful, take their young away from their nests, and oblige them to labor in slavery." (P. 72, Revised edition, 1859.)

Are we not struck with wonder and astonishment in thus beholding a slave-making power exhibited as an *instinct* amongst this lowly organized species?

Here we find this power developed to as great a state of perfection in these small and insignificant creatures of the earth,

as it has ever arrived at in the highest order of animals—man. Many readers, in perusing this account of slavery among ants for the first time, may feel an indignation at the thought, but a moment's reflection should satisfy them that these humble and scarcely observed insects are God's creatures, and by him endowed with the peculiar instinct which is doubtless exercised for the good of both species.

The almost equally wonderful cell-making instinct of the hive-bee next claims the attention of our author. The hive-bee has, as he asserts, practically solved a recondite problem of the mathematicians, viz: by making their cells of the proper shape to hold the greatest possible amount of honey, with the least possible consumption of precious wax in their construction. Without entering into the minutiae which our author has given on this subject, we may state, in brief terms, that he regards the perfection of this instinct, as also the slave-making instinct in ants, to be the result of a continued series of improvements upon what was originally a mere rudimental development. "I believe," says Mr. D., "that the hive-bee has acquired, through natural selection, her inimitable architectural powers."—P. 202.

Chapter the eighth forms an interesting essay on *Hybridism*. Our author's views on this subject are much in accordance with those of Dr. Morton, viz: that hybridity is not a *unit*, but exists in degrees according to the relationship between the parent species—they being classed as remote, allied and proximate. Mr. Darwin says, that we must not look upon the sterility of many hybrids as a special endowment, beyond the province of our reasoning powers, for the purpose of preventing confusion in species, but simply as the result of a functionally impotent condition of their reproductive organs, as may be clearly seen in the male element in both plants and animals, though the organs themselves are perfect in structure. The sterility that results from the sexual connection of remote species is, of course, not attributable to any imperfection on the part of either, but rather to the essential incompatibility of their propagating elements. In the discussion of hybridity

and sterility the author considers this distinction as highly important.

Our author, in common with Walker, and many other eminent observers, feels justified in believing, from the large body of facts which he has collected, that "close inter-breeding lessens fertility, and, on the other hand, that an occasional cross with a distinct individual or variety increases fertility." He thinks that this fact explains, in a measure, the infertility of many hybrids, it being well known that the majority of experiments have been practiced with brothers and sisters.

After speaking of the fertility of the hybrids from the common and Chinese geese, (*A. Cygnoides*), which are generally ranked as *distinct genera*, when crossed with either pure parent, and in one instance where they have bred *inter se*, he refers us to the origin and crossing of dogs:

"I believe, for instance, that *our dogs have descended from several wild stocks*; yet, with perhaps the exception of certain indigenous domestic dogs of South America, all are quite fertile together; and analogy makes me greatly doubt whether the several aboriginal species would at first have bred freely together and have produced quite fertile breeds."

This result, he maintains, has been effected by domestication. The same reasoning precisely applies to the fertility between the various species of men, as it has been applied by Morton, Nott, Agassiz, and others. The great body of ethnological writers, particularly the opponents of this principle, agree in classing man with the list of domesticated animals. Domestication and civilization, as terms used in referring to the *genus homo*, may be defined as almost synonymous; and it is well known that the fertility between the more highly civilized species is much more complete than that which follows the intercourse between the cultivated and barbarous species.

We will conclude our brief notice of this interesting chapter by giving the following extract, which we agree with fully, and endorse heartily. After having dwelt upon the degrees of fertility existing between various species of animals and plants, he says:

“Considering the several rules now given, which govern the fertility of first crosses and of hybrids, we see that where forms which must be considered as good as distinct species are united, their fertility graduates from zero to perfect fertility, or even fertility, under certain conditions, to excess. That their fertility, besides being eminently susceptible to favorable and unfavorable conditions, is innately variable. That it is by no means always in the same degree in the first cross, and in the hybrids produced from this cross. That the fertility of hybrids is not related to the degree in which they resemble in external appearance either parent. And lastly, that the facility of making a first cross between any two species is not always governed by their systematic affinity or degree of resemblance to each other. This latter statement is clearly proved by reciprocal crosses between the same two species; for, according as the one species or the other is used as the father or the mother, there is generally some difference, and occasionally the widest possible difference, in the facility of effecting an union. The hybrids, moreover, produced from reciprocal crosses, often differ in fertility.

“Now do these complex and singular rules indicate that species have been endowed with sterility simply to prevent their becoming confounded in nature? I think not. For why should the sterility be so extremely different in degree, when various species are crossed, all of which we must suppose it would be equally important to keep from blending together? Why should the degree of sterility be innately variable in the individuals of the same species? Why should some species cross with facility, and yet produce very sterile hybrids; and other species cross with extreme difficulty, and yet produce fairly fertile hybrids? Why should there often be so great a difference in the result of a reciprocal cross between the same two species? Why, it may even be asked, has the production of hybrids been permitted? to grant to species the special power of producing hybrids and then to stop their further propagation by different degrees of sterility, not strictly related to the facility of the first union between their parents, seems to be a strange arrangement.”—Pp. 229, 230.

This view of hybridity, which was put forth by Morton, in 1847, in “*Silliman's Journal*,” and afterwards extended in the *Charleston Medical Journal and Review*, up to the period of his death in 1851, our readers will perceive is essential to Mr. Darwin's theory of the origin of species. If we adopt his opinion, that many species originate in a common centre, we,

of course, would expect to find a large degree of fertility existing between these intimately related species. We subscribe to the author's views on hybridity, not because we believe with him that species are simple varieties of a single stock, but from our belief that hybridity is not a test of species when species are regarded as "*primordial forms*," as ably advocated by Morton, and afterwards by Nott and Gliddon.

Next in order we take up the consideration of chapter the ninth, which may be regarded as one of the most important in the book,—it being the one, in fact, which constitutes, in a great measure, the basis of the author's entire theory: it is upon the "*Imperfection of the geological record*." All must acknowledge and deplore this imperfection. Mr. D. accounts for the absence of the connecting links, or intermediate varieties, between the orders, genera and species now existing, by assuming that that they have, in process of time, become extinct; and the reason assigned why their fossil remains have not been discovered is, that the geological record is still in a state of great imperfection. He contends that these remains do exist, but, for the want of greater research, have not yet been found.

To our imperfect knowledge of the geological record must also be attributed our very inadequate ideas of the lapse of time. Speaking of this fact, our author says:—

"Independently of our not finding fossil remains of such infinitely numerous connecting links, it may be objected, that time will not have sufficed for so great an amount of organic change, all changes having been effected very slowly through natural selection. It is hardly possible for me even to recall to the reader, who may not be a practical geologist, the facts leading the mind feebly to comprehend the lapse of time. He who can read Sir Charles Lyell's grand work on the Principles of Geology, which the future historian will recognize as having produced a revolution in natural science, yet does not admit how incomprehensibly vast have been the past periods of time, may at once close this volume."—Pp. 247, 248.

By accepting as a fact, that geologists have but very partially examined the depths of the earth, and by allowing a

vast period of time for the operation of the organic laws of nature, which many of the leading authorities of the present day most unqualifiedly do, we are led to look upon Mr. Darwin's theory of the origin of species as possessing much more plausibility than when viewed in other lights.

After referring to particular instances showing the effects of time, our author gives the following computation:—

“Hence, under ordinary circumstances, I conclude that, for a cliff 500 feet in height, a denudation of one inch per century for the whole length would be an ample allowance. At this rate, on the above data, the denudation of the world must have required 306,662,400 years; or say three hundred million years.”—P. 251.

The most far-reaching and comprehensive human intellect could but faintly conceive the vast and manifold changes that would naturally and incidentally occur in organic and inorganic matter even in the limited period of one hundred million years. The mind of man stands awed and humbled in view of such a retrospection. What are we but, as it were, the butterflies of a summer day, that flaunt our colors in the sunlight for a few short hours, and then appear on earth no more!

Our author acknowledges, however, with his characteristic candor, that “all the most eminent palæontologists, namely, Cuvier, Owen, Agassiz, Barraude, Falconer, E. Forbes, &c., and all our greatest geologists, as Lyell, Murchison, Sedgwick, &c., have unanimously, often vehemently, maintained the immutability of species.”

While we have these authorities standing in opposition to the view of the origin of species by natural selection, may we not well adhere to them, and wait for further developments before accepting the novel theory?

Mr. D. adds to the above-quoted paragraph, that he has reason to believe that one great authority, “Sir Charles Lyell, from further reflection, entertains grave doubts on this subject” (immutability of species).

Chapter the tenth is kindred to the last, and is also highly interesting; but we have only time and space to give the heads

of the subjects treated. They are as follows: On the geological succession of organic beings; on the slow and successive appearance of new species; on the different rates of change; species once lost do not re-appear; groups of species follow the same general rules in their appearance and disappearance as do single species; on extinction; on the simultaneous change of forms throughout the world; on the affinities of extinct species to each other and to living species; on the state of development of ancient forms; on the succession of the same types with the same areas, &c. All of these topics are ably treated, and are, of course, adduced in further confirmation and illustration of the author's theory.

The eleventh chapter is headed "Geological Distribution." Although acknowledging "the extreme difficulty in understanding how the same species could possibly have migrated from some one point to the several distant and isolated points where now found," the author fully believes that all species originated in a single centre of creation, from which they have been gradually distributed to their present localities. He assumes that continents, and smaller bodies of land, now separated by water, constituted in former ages continuous areas; and thus explains also why we find identical species existing on different continents, without means of communication at any period within historical times.

Here, too, if we agree with the many authorities as to the great age of the earth, and we acknowledge that the evidence and the probabilities decidedly favor such a belief, we cannot positively reject or controvert this view of the distribution of species; for no mind can possibly place any limit to the changes that might take place even within the period of three hundred million years; but this assumption and argument is obviously placing the whole subject of origin of species, distribution, &c., beyond the reach of the human intellect, and leaves us entirely in the regions of wild conjecture. While the former popular belief of the age of the world—six or seven thousand years—was maintained, we could estimate with some degree of accuracy the changes that would probably occur

within that period, possessing as we do the experience of at least one-third of that time, and since the discovery of the ancient Egyptian monuments we are carried back two or three thousand years farther; but human experience, according to Lyell's chronology, would be but that of a day in the vast eternity of the past, and, of course could not be of the slightest value in estimating the evolutions that have been in active operation since the beginning of time.

Our author suggests various ingenious methods by which the dispersal of the different animals, insects, plants, etc., from a single centre may have taken place, which are supported by some show of fact; but still, his arguments have not induced us to abandon Prof. Agassiz's theory of "geographical distribution," as set forth in the *Christian Examiner* in 1850, and as also further elucidated by his paper in the *Types of Mankind* in 1854.

It is freely admitted by our author that there are many grave difficulties in the understanding of how the distribution in all cases could take place from a single point, but he believes that the preponderance of the evidence favors his theory, and that he is therefore justified in advocating it.

The twelfth chapter forms a continuation of the same subject; and, although it contains much valuable matter, we are compelled to pass on to a hasty conclusion of this condensed notice. The two remaining chapters, the thirteenth and fourteenth, are respectively headed "Classification," and "Recapitulation." With two or three extracts from the latter, and a few remarks, we will conclude our very imperfect review of this novel but instructive volume.

Referring to the probable reception that his opinions will meet with, our author says:

"Although I am fully convinced of the truth of the views given in this volume under the form of an abstract, I by no means expect to convince experienced naturalists whose minds are stocked with a multitude of facts, all viewed from a point of view directly opposite to mine. It is so easy to hide our ignorance under such expressions as the 'plan of creation,'

‘unity of design,’ &c., and to think that we give an explanation when we only re-state a fact. Any one whose disposition leads him to attach more weight to unexplained difficulties than to the explanation of a certain number of facts, will certainly reject my theory. A few naturalists, endowed with much flexibility of mind, and who have already begun to doubt on the immutability of species, may be influenced by this volume; but I look with confidence to the future, to young and rising naturalists, who will be able to view both sides of the question with impartiality.”—P. 417.

The author’s views of the origin of species lead to the following conclusions :

“I believe that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number.

“Analogy would lead me one step further, namely, to the belief that all animals and plants have descended from some one prototype. * * * Therefore, I infer from analogy that probably all organic beings which have ever lived on this earth have descended from some one primordial form, into which life was first breathed.”—P. 419.

What a stupendous supposition! What an overwhelming thought!—not only that Man, the most complete and perfect of all organized beings, is an improvement, through progressive development, upon the monkey and orang, as was maintained by some of the older writers, but that he is actually, and only, an improved and highly cultivated Molusca or Radiata,—that his origin is identical with, not only that of all other animals, but with all plants! His pride may well be humbled in view of such a possibility. Mr. Darwin is right in believing that the world is not yet prepared for the reception of such an astounding idea.

Although we have long considered the human genus as but one step above the Chimpanzee and Gorilla, we have always viewed them as of essentially different orders, and of essentially different origins. We even believe (as we expect more fully to discuss at a future time) that there are various species of man, each having an independent origin; therefore, in the

present state of our knowledge, and in our present mind, though we believe that there is much truth in many of the author's speculations, and that they may be studied with profit, we are yet utterly unable to accept or agree with the distinguished writer's remarkable theory of the origin and development of species.

ART. IV.—(*Passage of a large Stone through the Urethra of a Boy two years old.*) A case treated by Drs. W. CLAIBORNE and (W. M. TURNER, of Petersburg, Va.) (Reported by Dr. TURNER.)

ON the night of the 29th ult., I was sent for to see a boy who, the woman told me, was suffering from retention of urine. The case was under the treatment of Dr. Weldon Claiborne of this city, and the woman who called me brought a request from Dr. C. that I would carry a catheter with me and endeavor to get away the urine; he, the Dr., being compelled to go in the country before returning to his office, and he had, unfortunately, no catheter with him. I went and found for my patient a handsome little boy, (colored,) about 27 months old. He was lying on his back, moaning most piteously—his countenance betraying acute suffering. The pulse was both frequent and quick, ranging in beats to the minute from 125 to 135. Before proceeding to explore the region of the bladder, I was led to examine the chest by the circumstance of accidentally placing my hand over the right lung, and feeling a decided vibratory thrill. Applying my ear for ausculting purposes, I detected a pneumonia in the upper lobes of both lungs, and in the *second* stage; as bronchial respiration, and bronchophony were well marked, as well as was dullness on percussion. Upon inquiry, I learned that the child had been sick a month previous with measles, and, having taken cold when

recovering, had been complaining more or less ever since that time.

The bladder, to which I then turned my attention, was enormously distended, protruding and hanging over the pubic arch, somewhat resembling in miniature the "*pendulous-belly*" of pregnant women. The epidermis of the abdomen over the bladder was quite red and shining, and I was indeed fearful of an artificial opening. The penis was much enlarged, and apparently infiltrated with urine. This, however was not the case, as subsequent examination proved. To add to the difficulties of the case, there was present a congenital phymosis—rendered ten times more rigid in its contraction by the swollen state of the penis. It was a difficult task, which I then set to work to accomplish—that of drawing off the urine of a boy of 27 months old, under circumstances of such pain to the little patient, and of such disadvantage to myself. The swollen and phymosed condition of the penis, and the great irritability of the child, rendered it almost impossible even to make an attempt with the catheter. At the slightest touch the boy would shriek and scream, until I actually thought the strained bladder would rupture before the prolonged muscular contraction would relax. Finally, after much patience and considerable manoeuvring, my only assistant being an old lady, who managed to hold the child's hands, I succeeded in passing a small gum catheter through the hard, pursed-up phymosed ring, and, as fortune would have it, into the urethra. This *feat* I was compelled to perform several times, for the frantic efforts of the child dislodged the catheter, and my labor was thrown away. However, I placed my knee upon the lower extremities of the child, and in this position succeeded in passing the instrument once more into the urethra. Moving it slowly and cautiously downward, I soon had the satisfaction of reaching the bladder, which I knew to be the case by the sudden gush of the pent-up urine through the catheter. At the neck of the bladder, I noticed a slight *gritty* feel as the catheter glided along; it was transient, however, and I thought nothing of it, attributing it to the ring of a cartilaginous stricture, which I had no doubt

existed. The boy was instantly relieved, and fell asleep while the urine—an enormous quantity—was flowing. I prescribed nothing for the patient that night except cold cloths around the penis. The next morning, the first of August, I saw the boy with Dr. Claiborne; we found the patient much refreshed and considerably improved by a good night's rest. The belly, over the region of the bladder, had lost the prominent *bulletry* appearance of the previous evening, and appeared more natural, yet it was still very tender. The penis possessed yet its infiltrated look, and resembled much a transparent gelatinous substance—differing only in a *solid appearance*. We found that the boy had already passed his urine once, but in a very unsatisfactory manner, screaming at the top of his voice when the urine reached the urethral canal, and refusing most positively to make further effort. The result was, when we arrived, he was again unable to urinate. Once more my little gum catheter was called into requisition, and our united efforts had the desired effect of bringing away the urine. As I had a case of instruments along, Dr. C. advised operation for phymosis; suggesting that, perhaps, the rigidity with which it fastened around the glans-penis prevented micturition. We divided the pressure with a bistoury and director. The operation was over in a few minutes. After splitting the prepuce, we discovered that the retention of the urine could not have been due to the pressure made over the mouth of the urethra by the phymosis. When I passed the catheter in to empty the bladder, I detected very plainly the same *gritty* feel which I had remarked the evening previous. Dr. Claiborne was equally sensible of the peculiar sensation. The touch was decisive; there could be no mistaking the fact of the presence of a foreign body. Dr. Claiborne immediately pronounced the case to be stone. In withdrawing the catheter, which I had used as a sound, I distinctly felt an opposing substance. Dr. C. at this moment remarked, that he distinctly felt, between his thumb and fore-finger, a round or irregularly round tubercle in the urethra. After several introductions of the catheter, I succeeded finally in hooking the stone in the fenestra of the

instrument, and, with a sudden motion, drew it forward an inch toward the external orifice of the *mentus urinarius*. Here we experienced much difficulty in our work; we dared not split the urethra; and yet it was very difficult to obtain a hold on the stone, which showed a constant tendency to retrocede. Finally, after much patience and manipulation, we obtained a fastening on the stone with a pair of dressing forceps, and extracted it. The boy came near swooning, but soon revived. The following are the dimensions of the stone: Length five-eighths, and circumference three-quarters of an inch; weight nearly forty-two grains; shape, semi-conoidal and semi-cylindrical—the presenting end corresponding to the apex of the cone. An analysis of the stone proved it to be oxalate of lime.

A further examination, using the catheter as a sound, revealed, as we had already suspected, the presence of another calculus in the bladder, and we came to the conclusion that the lithogenesis was hereditary. On inquiry, however, I learned nothing which would warrant the supposition. The boy has not suffered, especially since the time of the passage of the stone. He is now running about, and seems to be lively enough. That he will be troubled again with calculus, there can be no reasonable doubt; how soon first, I cannot say.

The noticeable feature in this case is, that a *stone of such a size* did pass through the urethra of a child two years of age.

ORIGINAL TRANSLATIONS.

I. *Experimental History of Recurrent Sensibility*. From the French of Claude Bernard. (Translated for the Maryland and Virginia Medical Journal.)

Gentlemen,—We have established in the last lecture what we understand by nervous unity, and how it is necessarily composed of two elements—the one, motor; the other, sensitive—which, however, notwithstanding their different character, are found intimately connected by a common property.

Now, that we have indicated the signification which must be given to recurrent sensibility, we will show immediately the experiments which establish it. But as there exists here one of the most important of nervous phenomena, and one of those fugacious properties which, by its absence or its presence in different cases, has led to contradictory appearances in the results of experiments, I desire to make known to you, by the same process of discovery, all the difficulties of this prominent question. We will give at the same time an account of other nervous properties whose recurrent sensibility is inseparable.

I have already told you what difference existed between the functions of posterior branches charged with transmitting the sensitive impressions, and posterior branches charged with transmitting the motor excitation. Chas. Bell advanced an opinion by induction, about the commencement of the present century, that the posterior branches should be sensitive, and the anterior branches motor. You know that, still later, Magendie found, on cutting each separately, that on this point the views of Chas. Bell were correct. So much for functions. But Chas. Bell has gone yet further, and has advanced theo-

retically views on the properties of those organs, which have been determined to be incorrect. Now, while no one discusses further the functional *rôle* of the nervous branches, their properties remained the subject of a very brisk controversy. Agreeing with Chas. Bell, so far as regards the function of the anterior and posterior branches, Magendie, on discovering recurrent sensibility, ceased to consider the properties of the nervous element. The English physiologist thought, indeed, that the posterior sensitive branches were sensible, whilst the anterior motor branches were insensible—a theoretical view which you already know to be incorrect; the two branches are sensible; but their sensibility proceeds from the same source. In 1822, Magendie, wishing to submit to experiment the views of Chas. Bell, found the anterior branches sometimes sensible, sometimes scarcely sensible, sometimes completely insensible, while the posterior branches were always sensible.

These experiments, having been repeated in 1839, gave results sufficiently positive for Magendie to conclude that the anterior branches were sensible. Since 1822 he has made other experiments on the nerves of the face, and not only saw that the facial nerve was sensible, but also that it derived its sensibility from the fifth pair. Then, finding still later, that the anterior branch was sensible, he was also able to see that it derived its sensibility from the posterior branch, and that this sensibility came to it, not by the brain, but by the periphery. Although a series of experiments had perfectly established the existence of the facts he announced, the negative results before observed still remained. Later, they reappeared, even with persistence; there was a singularity about them which could not be explained. Magendie experimented on this same point. M. Longet, who was then attending a course at the College of France, frequented the laboratory and assisted in the experiments. He thought he could reclaim the idea of the discovery in letters addressed to the “*Gazette des Hôpitaux*.”

Some time after, M. Longet, after wishing to repeat the experiments of Magendie, no longer reclaimed anything; he pub-

lished, on the contrary, a work in which he established that recurrent sensibility did not exist. The contradictory results which Magendie had obtained in 1822, 1829 and 1839 caused him then to be very bitterly and unjustly reproached. The fact is, that Magendie had always proclaimed what he had seen, with an entire good faith and with a love of truth, without paying any attention to the varieties which the phenomenon might offer, and which the proof of different results of experiment showed. Yet, sad thing, and one well calculated to disconcert the experimenter in the posterior experiments, in 1839 Magendie and some others could no longer discover recurrent sensibility, and it was thought its non-existence must be definitely admitted. However, one phenomenon, which has been once observed, exists.

When contrary results finally happen, they in nowise destroy the first; and in the presence of the uncertainty which they may throw on *too affirmative* conclusions, the way to arrive at the truth is not to gainsay positive results on account of negative results or reciprocally, but rather to search out the cause of their divergency.

At this time (1839,) I followed the course of Magendie, and I also frequented the laboratory. I had been witness of the facts which he had signalized, I had seen them before me, I had touched them, and although in his new experiments, Magendie might no longer have found the sensibility of the anterior branches, I could not on that account admit that what I had seen did not exist. Still later, I repeated myself, in divers attempts, experiments on recurrent sensibility in the course of physiology which I delivered, and for several years I did not find it. I discarded, then, the idea of a fugitive property, whose conditions of existence had to be determined. But it was not until four years afterwards, when the question seemed to be agitated, whether anterior branches were generally regarded as insensible, that my attention was directed to the different conditions in which the experiments had been made. I recollected that, in 1839, when I followed the course of Magendie, the experiment was made in the morning; and

it was only in the amphitheatre, after having allowed the animal repose, that they sought for the sensibility of the anterior branches—a sensibility which was always very evident. In experiments made later, at a medical session, the anterior branches, on the contrary, were pinched *immediately after being exposed*, and they were found to be insensible. I thought, then, that the promptness in this case was a condition of success. After my last reflections on the situation of animals among whom I had seen recurrent sensibility existing, the contrary condition appeared to be the result of the facts observed; for, in this case, the animal had been able to enjoy, after the experiment, several hours of repose, which, in dissipating the fatigue of the operation, caused it sensibly to return to the conditions of its normal state. Another consideration which gives its aid to this observation—the facial nerve is sensible; when it is cut, the peripheric end is invested with a recurrent sensibility, which is never wanting. Why? Because it is easy to cut the facial nerve without fatiguing the animal; because a simple incision brings us to the nerve, an operation which could not be compared to the opening of the vertebral canal. My attention being directed to this point, I noticed that in an animal, the vertebral canal of which had just been opened, the peripheric end of the cut facial nerve was insensible. If then, in operating on the rachidien nerves, we do not find the anterior branches sensible, it is attributable to the pain and fatigue of the operation having brought them to a state of exhaustion, which considerably diminishes the sensibility. Now, at the commencement of the operation, we observe that the animal cries at first when the skin is cut and when the muscles are divided; later, when the vertebral canal is opened, we can pinch the skin, we can sew it up, without causing the animal to cry. Is it necessary, then, to conclude that the skin is insensible? Sensibility disappears instantaneously in an animal subjected to a severe cause and sudden exhaustion; certain parts lose their sensibility before others; this is the only explanation. At a given moment, sufficiently soon after the operation, recurrent sensibility may then be

completely wanting, while an hour after, it exists very appreciably. It is then a mobile property, subject to such oscillations that we can appreciate the conditions of the phenomenon better in the normal state, which is the most feasible reconciliation of facts. Of all the organic systems, the nervous system is the one which suffers most from exhaustion. Chloroform acts in the same way, and I have already told you that, under its influence, recurrent sensibility disappeared but to return when the effects of the anæsthetic are dissipated. This influence of exhaustion is at present well established, and we can foresee sufficiently exactly whether, in a given case, we may or may not find signs of the sensibility of the anterior branches.

Experiment in regard to recurrent sensibility is much easier, and the results are much clearer when we practice on a vigorous, well nourished animal. It is chiefly upon dogs that we have made our observations; they undergo the operation best. Cats may be employed, although it is more difficult to isolate the branches. I have never once succeeded with rabbits; these animals nearly always die during the operation; it is the same with horses, which, if they do not die, are always too much exhausted to preserve the sensibility of the anterior branch.

We are now going to unfold to you the experiments which we have been obliged to pass over in order to arrive at determining the conditions of the existence of recurrent sensibility. We will not relate to you all the experiments we have made; we will signalize the principal ones. Notwithstanding that, the narration will be quite lengthy. But we wish to make you assist, so to speak, in the experimental evolution of the question, and to show you the immense variety one physiological phenomenon can offer. Here, then, is a first series, in which recurrent sensibility was wanting, because we sought for it immediately after the operation, without taking into consideration the shock sustained by the animal. The experiments of Magendie on recurrent sensibility were made in 1839, and the contradictory experiments of M. Longet were made in 1840, and published in 1841. It was at this time that we undertook

on our side experiments to seek again for recurrent sensibility. We opened largely the vertebral canal on both sides, and we took care, as we have before said, to examine the branches as soon as they were exposed, thinking (wrongly, as we have since learned,) to take them thus in the conditions the nearest approaching the physiological state.

Experiment, (16th of April, 1841.)—A large sized dog, aged ten or twelve months, healthy. The lumbar region of the spinal column is greatly exposed; the animal has lost much blood. He has preserved, nevertheless, mobility and sensibility in the posterior members. We separated alternately the anterior branch into four pairs, and pinched it without giving rise to the least pain. The result was the same with the four branches. The corresponding posterior branches were endowed with a marked sensibility, which persisted in the central end when the branch was cut.

Experiment, (19th April, 1841.)—Large sized dog, aged eight months. The spinal marrow was exposed on both sides in the lumbar region. The experiment was made very rapidly. Six anterior branches were laid bare and isolated, three on each side. Each of these anterior branches were successively pinched; there was no pain occasioned in any of them. Only a slight muscular trembling was observed in the member when the anterior branch was at first compressed. On a second nervous pair, we cut the anterior branch, leaving the posterior intact. At the moment of the section of the branch, no pain was manifested. But there was at this moment a strong contraction in the muscles of the member, as if produced by electrical excitation. Both ends, after their section, were successively pinched, and found to be completely insensible. The corresponding posterior branch was very sensible. On the third rachidien pair, the posterior branch showed on pinching a very marked sensibility. The anterior branch did not show any. A section of this branch was made; there was at this moment a violent contraction in the muscles. Two ends resulting from this section were completely insensible. On a fourth rachidien pair, the posterior branch which was composed, as it

were, of two bundles, was very sensible, whilst the anterior branch was completely insensible; pinching produced only a slight contraction in the muscles of the member. On a fifth pair, we made the section of the anterior branch, leaving the posterior intact. Pinching both ends of the anterior branch gave no pain. Pinching of the posterior branch provoked only an obtuse pain, which doubtless existed from the time of the laying bare of the spinal marrow; it was, in fact, nearly an hour after this had been effected. The animal was nevertheless still vivacious. On a sixth rachidien pair, the anterior branch was found wholly insensible, and the posterior branch was dully sensitive. We pierced then the anterior bundles of the marrow with a cataract needle, which were found to be completely insensible, whilst the sensibility of the posterior bundles still persisted, but considerably lessened.

From 1841 to 1844 analogous experiments have often been repeated, and always without discovering recurrent sensibility, which was sought for in the same conditions as in the preceding. It was in 1844 that the idea occurred to us, that the conditions in which we were placed, might be different from those in which Magendie had observed recurrent sensibility, and that probably it was necessary to let the animal repose, instead of experimenting immediately on the rachidien branches. After these new views, we undertook other trials, which, however, did not succeed at once, as may be seen by the following experiments:

Experiment, (30th September, 1844.)—On a full grown dog, very vigorous, the lumbar portion of the vertebral canal was largely opened. The operation scarcely lasted half an hour, but the animal lost a great quantity of blood. The motions of the posterior part of the body were all the time very free, and as soon as the animal was let loose, he was able to escape. An hour and a half after the operation, the animal was in the same state; but the posterior train appeared less sensible, and very violent pinchings had to be exercised to provoke cries of pain. The state of the rachidien nerves was then examined. The posterior branches were slightly pinched, and found to be endowed with great sensibility. All the time, when we merely

touched them, there was no pain, as was the case among many animals. We then pinched the anterior branch, taking care to leave the posterior branches, which corresponded to them, intact. They were found to be completely insensible. It is necessary to add, that before pinching this branch on the right side, we previously cut two or three posterior lumbar branches on the left side. The animal appeared a little enfeebled, and there was no reaction in the wound.

Experiment, (24th September, 1844.)—On an adult dog, middling stout, very active, the lumbar spinal column was opened. Immediately after the experiment, the posterior portion of the animal was a little benumbed, and he staggered on his legs, especially to the right side. Yet both pairs remained sensible. Little by little the numbness disappeared. The animal supported himself well on his feet and was saved, although the posterior right foot was always evidently more feeble than the left. When the animal finally stopped walking, he did not touch the ground with his posterior right foot, which he held bent and elevated. The wound in the back was sewed up, and the animal was allowed repose till the following day. Then (25th September) the animal was lying down, sad and drowsy; he had eaten nothing. When he walked, his posterior part was stiff, and the movements less easy than on the previous evening. The weakness remained all day—greater in the right foot. We re-opened the wound, cleansed it, and examined the state of the branches. We then found, on the right side, that the posterior branch of the third lumbar pair had been cut in the operation, very near its ganglion; the corresponding anterior branch was intact. We pressed this anterior branch which showed no sign of sensibility, whilst the end of the posterior branch attached to the spinal column was very sensible. In raising a little the spinal marrow by pinching the dura mater, we disengaged on the left side an anterior branch, and pinched it without obtaining any sign of sensibility, whilst the corresponding posterior branch as also the others were very sensible.

Then the animal was poisoned with the alcoholic extract of *nux vomica*, introduced with a reed into the right axilla. At

the end of ten minutes tetanic convulsions were manifested, and it was at this time that the side of the marrow and the rachidien branches could be observed. When the convulsions commenced, and a little before their appearance, the sensibility of the marrow and of the posterior branches was evidently exaggerated, and the slightest touch, which before determined nothing but a slight reaction, actually brought forth cries and violent actions. On account of this excessive excitation of the parts, it was impossible to isolate conveniently the anterior branches, to see if they had taken on sensibility. At the moment when the convulsions and the tetanic rigidity existed, the spinal marrow, the posterior branches, touched or pinched, seemed completely insensible. Perhaps it was owing to this that the convulsed limbs could not react in order to give any sign of pain.

From 1844 to 1846 we could not avail ourselves of experiments. It was only at the beginning of 1846 that we recommenced a series of new researches which, after a succession of gropings and trials, led us to recognize the conditions of the experiment, and permitted us to establish then the mode of action to which we should pay attention, in order to place the animals under such conditions, that the circumstances of the operation itself should not be injurious to the physiological development of the nervous phenomena which we wish to observe. As this excites one of the most delicate questions concerning the nervous system, I desire that this instruction should be given you, as we ourselves have acquired it, that is to say, by the direct observation of facts; it is on this account that I am going to relate to you, although it may be a little tedious, the series of experiments we have made, in order to arrive at the determination of the conditions of the existence of this nervous property, which presents this singular particularity of having been entirely lost, after having been found and well determined. We will follow, in this recital, chronological order, and you will see, in studying a variety of obtained results, and in seeking out their relations with the conditions in which we found them, that we have been able to deduce from them definite conclusions.

Experiment, (10th January 1846.)—On a young dog, about two months old, very active, healthy digestion, we opened the vertebral canal in the lumbar region. The operation lasted about twenty minutes, but the animal lost much blood. An instant after the operation we pinched successfully the anterior branches, which betrayed great sensibility. This sensibility was always greater in the branches first pinched, and the animal, all the time losing blood, was enfeebled: sensibility became very obtuse, and even doubtful towards the end of the operation. A little while after the animal died of exhaustion, or rather of hemorrhage.

Experiment, (23d January, 1846.)—On an adult dog we opened the vertebral canal in the lumbar region; the operation was very long; the animal was weakened by it. Immediately after the operation, we pinched the anterior branch, and the animal seemed to experience a sensation, but it was confused. The posterior branches were, on the contrary, endowed with a lively sensibility.

Experiment, (26th January, 1846.)—On an adult dog—in digestion—we opened the spinal marrow. The experiment, very laborious, lasted an hour; the animal, which was very much fatigued, rested lying on its side. We allowed it to repose, after closing the wound; he revived gradually; it was not until two hours afterwards that we examined the condition of the branches. In pinching an anterior branch on the left, there was, as generally, motion of the member; but the animal appeared, at the same time, susceptible of pain. Several branches presented this same result; only it was on pinching the first branch that the pain showed itself more marked.

Experiment, (28th January, 1846.)—On an adult dog who had not eaten anything for three or four days, we opened the vertebral canal in the lumbar region. The experiment was sufficiently long (lasting about half an hour), and the animal was very much fatigued, although he had not lost much blood. The anterior branches examined soon afterwards, showed no sensibility, while the posterior branches gave very evident signs of sensitiveness, but less marked than usual. The ex-

haustion of the animal still increased, and sensibility became more and more obtuse in the posterior branches. When they were pinched, the animal no longer uttered cries, and did not manifest by any motion the presence of pain.

After the experiments above given, we may already remark, that among the old dogs enfeebled, either by anterior experiments, or by the length of the operation, or by a starved condition which rendered them still less able to endure the operation, the depressed recurrent sensibility was not seen till late, and sometimes not at all; whilst we have met with it almost immediately among young dogs, active, in good condition, and who were better able to stand the exhaustion consequent on the operation. But we should ask ourselves if, among animals where sensibility has not been determined, it might not have disappeared, simply by the fact of the exhaustion produced by the opening of the vertebral canal.

It has been difficult to decide this question directly, because it would be necessary, in order to do so, to be able to examine the recurrent sensibility of a similar branch both before and after opening the vertebral canal. Yet we may arrive at the decision of the question indirectly, and find out if the opening of the vertebral canal could by itself render the motor nerve insensible; and that may be done by taking another nerve—the facial, for example—whose sensibility (recurrent), should be examined, both before and after the operation of opening the vertebral canal. It is for this reason, that in the following experiments we have sometimes examined the sensibility of the facial, in concurrence with that of the rachidien branches.

Experiment, (12th February, 1846.)—On a young dog, two months old, in good condition, very active, the vertebral canal was opened; the orifice was as small as possible, and we raised only two arches of the vertebræ. Soon after we pinched an anterior branch, taking care to leave intact the corresponding posterior branch. Pinching the anterior branch produced evident pain, which we determined very decisively in two different attempts. On this same animal we laid bare the facial nerve, and pinching the three branches of the nerve, we found them

all sensible. Then we divided them, and pinched successively the peripheric ends. Two ends, the inferior and the middle, appeared more sensible than the superior, which was only so in a doubtful manner.

Experiment, (13th June, 1846.)—On a dog about six months old, middling stout, well fed, having drunk milk an hour before. The opening of the vertebral canal was very difficult; the bones were hard, and the spinal marrow was opened a little above the lumbar curve, somewhat higher than usual. Then wishing to separate the dura mater from the fat which surrounded it, the animal made a sudden movement by which the dura mater was punctured. A certain quantity of cephalo-rachidien liquid flowed from the puncture. It seemed that after the flow of this liquid, the animal was more irritable than before. Then we isolated an anterior branch, pinching it, and leaving the posterior intact. It was very evident that the animal suffered pain. It became very difficult then to re-experiment on the same branch: the animal being turbulent, was scarcely manageable. Yet we could determine that the anterior branch which was sensible, had preserved its sensibility in the peripheric end, above the point first pinched. We then made fruitless attempts to arrive at determining the properties of the anterior branch on the opposite side, because the agitation of the animal offered an obstacle to it. It was then decided, to render the operation easier, to open more largely the vertebral canal, by raising higher the arches of two vertebræ. Immediately after this new operation, the animal sank rapidly, and instead of being turbulent as before, he became perfectly calm. Then the anterior branches could easily be isolated; but when pinched, they showed not the slightest trace of sensibility. Then we laid bare the branches of the facial nerve, and found them completely deprived of sensibility. The posterior lumbar branches preserved their sensibility all the time.

Gentlemen, all the experiments which we have made known to you, up to this time, lead us gradually to the knowledge of the conditions under the influence of which, recurrent sensi-

bility may disappear and re-appear, and we see *how much* these opinions differ from those we entertained, *à priori*. We had thought that it was necessary to act immediately after the opening of the *rhachis*, whilst, on the contrary, it was necessary to wait to allow the animals to repose a sufficient time after the operation, that they might recover from the fatigue and nervous excitement which the denudation of the marrow necessarily produced. We must not always wait too long ; for the next day, the animal becoming sick, and inflammation evading the tissues, sensibility may have disappeared.

BIBLIOGRAPHICAL RECORD.

I. *A Treatise on Gonorrhœa and Syphilis*. By SILAS DURKEE, M. D., Fellow of the Massachusetts Medical Society, &c., &c. (Pp. 442, 8 vo., with 8 colored plates.) Boston : John P. Jewett & Co., 1859.

THE Preface begins by informing the reader that, "in the year 1854, the author prepared an essay on "The Constitutional Treatment of Syphilis," which was honored by the award of a premium from the Boylston Prize Committee of Harvard University. That essay constitutes a large portion of the present volume ; and, although it has been modified to to a degree that deprives it of its original identity, it is believed that its intrinsic merits have been materially enhanced." And then with little more ado, chapter first is opened.

The first sixteen chapters of the work are devoted to the consideration of gonorrhœa ; while syphilis fills the remaining twenty-three chapters, which are illustrated. The design of

the work is eminently practical; so much so, that all extraneous ornament is carefully avoided, and the history of the two diseases, these lashes of Nemesis, entirely omitted; but, with the directness of purpose of one familiar with consultations upon these matters, the author proceeds to expose the parts.

The idea of the unity of the gonorrhœal and syphilitic poisons is at once very summarily disposed of; and this, we are of opinion, is much to be regretted in a work of such character and consequence, since a fair presentation of the two sides of the argument, while not unsettling the orthodox opinion of "the more enlightened members of the profession," could render an essential service to the less enlightened, and prove peculiarly acceptable to junior readers, by opening to them the entire field of discussion, and enabling them the better to understand the position which they are led to assume. And we more particularly insist upon this point, in connection with the initiatory of the 'treatise,' for the reason that the division of the two subjects very naturally follows from such a mode of treatment, and because theory and argument enter into other portions of the work.

However the bare verdict, very explicitly given, may, by some, be deemed conclusive without the evidence by which it was reached, nevertheless, we feel assured that a careful review of the question of the identity of syphilis and gonorrhœa would have proved both acceptable and edifying to student and practitioner. We expect to be brought up to the actual state of knowledge as to the singleness or duality of certain poisons, and are hardly content to be placed upon the most recently cleared level of science, without having become acquainted with the route that conducted us thither; although we admit that, in a manual, it is not absolutely indispensable to be informed what facts determined the belief of the unitarians, and what proofs satisfied the dualists, in order that the recognition and treatment of disease under the latter creed should be successful. The author, however, in affirming his position, makes a statement which we reproduce:

“For a very long period the identity of syphilis and gonorrhœa was a fruitful theme of debate; and opinions the most contradictory were entertained by the authors and schools thus engaged in the controversy. But the times have changed; and the dawn of a new and better epoch now opens upon the path of the medical practitioner. By the talents and the patient and well-directed labors of some of the most eminent writers that have come upon the stage during the last quarter of a century, the laws that govern the two diseases we are about to consider, have been brought from their long night of obscurity to the light of day, and are now comparatively well understood. * * * At the present day, the more enlightened members of the profession agree in the opinion that gonorrhœa is not a venereal affection; and yet, as gonorrhœa is usually contracted under circumstances identical with those that give rise to syphilis, a treatise on the latter disease, however judiciously prepared, would be regarded as deficient in design, unless the former complaint received a due share of consideration.”

In substantiation of the modern view, we find it stated on p. 11, that “contagious blennorrhagia is a specific disease,” and “cannot give rise to secondary syphilitic accidents;” and farther on: “One of the best arguments and proofs that can be offered against the identity of the syphilitic virus, and the virus of gonorrhœa, is, that the constant tendency of the first is to produce secondary symptoms, which we denominate constitutional syphilis; while it is equally true that gonorrhœa, however severe, or however protracted in duration, is never followed by such symptoms.” “The opinion that simple blennorrhagia, without the existence of chancre in the urethral canal, may be followed by constitutional manifestations, such as well-marked, rounded ulcers on the tonsils, mucous tubercles at the commissures of the lips, about the arms, on the scrotum, syphilitic ecthyma, papular eruptions, &c., is not sustained by the history of the disease. There is no well-authenticated example which proves that an individual, in whom the mucous surfaces have been examined during an attack of blennorrhagia, without the existence of chancre, has ever afterwards had secondary syphilitic lesions in any form.”

The consequences and sequelæ of true gonorrhœa are next

enumerated, and declared, of course, to be "non-venereal" (or non-syphilitic); while a consideration of these subjects is reserved for a separate and appropriate portion of the treatise.

We cannot leave the chapter on gonorrhœa without expressing our belief that the views it maintains have enjoyed and will meet with the entire approval of the great mass of the profession. All who have ever given a careful attention to the causes of simple blennorrhagia and virulent gonorrhœa, must acknowledge the difficulties which sometimes beset the inquiry, as, for instance, the precise determination of the character of certain vaginal discharges in the accused female, the reliability of the testimony of the accusing male, and the size of the *granum salis* to be allowed to parties occupying various rounds of the ladder of suspicion, or, in certain cases, the mythical regions above. The intelligent author has felt the force of this observation; but we opine that he has, with equal justice, admitted the leucorrhœal discharge in different stages of preservation in the vagina, among the causes of simple gonorrhœa, or 'urethral running,' which do not necessarily fasten guilt upon the parties, and insisted upon the necessary impurity of the connection which is followed by contagious or virulent gonorrhœa in an individual previously in a state of health. We beg leave to offer our opinion, based upon professional experience, in favor of the production by blameless leucorrhœal matter of a purulent discharge in the male; and to state our doubts, formed in the same school, as to the exemption of any class or grade of society, as such, from pollution in the pool designated "The sinful lusts of the flesh."

Under the head of "Treatment," we discover no innovations upon the much explored field of gonorrhœal therapeutics, but regret the absence of chloride of zinc from the list of articles which in solution are injected into the urethral canal. This agent has been proposed, and it proves very efficacious in the earliest stages of simple running and "contagious gonorrhœa," by speedily arresting the discharge, or inducing a new condition of the mucous membrane, which responds favorably to the action of many of the mineral astringents employed in the form

of a very weak injection. But the remarks upon the abortive treatment of gonorrhœa by nitrate of silver are, we venture to assert, extremely judicious, and are certainly the expression of honest conviction derived from careful observation and a due regard for the "ample testimony" of other writers of merit. Thus we find on page 21: "If the (strong) injection be resorted to before the inflammatory stage has commenced, that is, before the patient complains of scalding in micturition, it will sometimes prove an efficient agent in at once cutting short the disease. Of this there is ample testimony from the highest sources; and there may be cases in private practice in which the measure is imperatively demanded. * * * Although the abortive treatment is productive of the most beneficial effects in certain cases that present themselves to the regular practitioner, it is manifest that the number of such must be comparatively small. If the discharge has existed for more than a day and a night before the patient applies for relief, it will be too late to make trial of the revulsive method; for the inflammation, in nearly all these cases, will be too acute to justify such a procedure. The favorable moment for a fair experiment has been lost; and if now the parties have courage and confidence to engage in it, they will but prepare the way for unwelcome retrospections."

Besides the risk attending this mode of assailing the disease, and the dangers awaiting its employment by the young and inexperienced surgeon, "who may be unduly influenced by the solicitations and suggestions of the patient," "the intense pain which always follows the application of the strong injection is a serious drawback upon its use. Another objection is, that in many instances it not only fails to benefit the complaint, but creates an aphthous condition of the mucous membrane, or sloughing ulcerations, which prove exceedingly troublesome, and are often more difficult to cure than ordinary gonorrhœa."

Death even has resulted from the the treatment in question. "Langston Parker mentions the case of a medical student who contracted a blennorrhagia, to cure which he used in the incipient stage a strong solution of nitrate of silver. An intense

urethritis followed, with pain in the abdomen and groins. The young man had peritonitis and an immense inguinal abscess, of which he died in one week."

"Some have thought, even to the present day, says the renowned Ambroise Paré in his marvellous *Œuvres*, that the *chaude-pisse* has something in common with the gonorrhœa of the ancients; but they are very different, as thou mayest see by this treatise; for gonorrhœa is an involuntary flux of semen streaming from every portion of our body to the parts of generation, caused by the resolution and paralysis of the retentive faculty of these same parts, as Galen says at the end of Lib. VI., *De locis affect*; or else of a too great abundance of blood and seminal matter within the body, which, not being transformed into fat and habit of body, takes its course towards the genital parts. On the contrary, the *chaude-pisse*, or *ardar urinæ*, is a sanies flowing from the penis, of a yellowish color, sometimes greenish, at others sanguinolent, approaching the quality of a more or less concocted pus, of a bad odor, with an acrimony which most frequently gnaws and ulcerates the urinary canal, causing painful erection of the penis and genital parts." In our day we designate the "involuntary emissions of seminal fluid, recurring at sufficiently frequent intervals to derange the general health," spermatorrhœa, with a perfect comprehension of the source of the potential fluid, and of the uses of the parts of the generative apparatus co-efficient with the testicles. It is a condition which demands the most serious attention of the surgeon, although, says Mr. Curling, "medical men are too apt to treat lightly the complaints of patients affected with spermatorrhœa, making no efforts to allay their anxiety—a course which often leads them to apply for aid in illegitimate quarters, and to become the victims of unprincipled men." Alone, its consequences are sufficiently terrible to awaken the most lively interest in the breast of any other than the frigid routinist; the clammy skin, the perverted affections, the fading health, the complete mental and physical marasmus that ends in destruction, all wait upon the car of this Juggernaut, dragging victims beneath its wheels; and, coupled with masturbation, it grins

and chatters at you, stalks by with vacant leer, or shakes its bars, phrenzied and despairing, in the abodes of insanity.

Without pausing over the graphic picture of the disease or its well judged treatment, we take occasion to renew our hostility to the nitrate of silver used in strength in the urethra, and in this feeling we are supported by many, if not most, of those surgeons whose talents, high position and faithful observations entitle their opinion to our fullest confidence. We cannot present the matter more fairly than by giving our author's own words:

* * * "In cases of long standing, in which irritation of the prostatic portion of the urethra has become a well marked feature, the application of the nitrate of silver sometimes answers a valuable purpose in removing the irritable and sensitive condition, or other lesion that may exist in the urethra, at the orifice of the seminal ducts, or in the follicle of the prostate gland. To overcome this sensitiveness and irritability of the parts, and then break the chain of morbid phenomena, is an important achievement; and it is claimed by the advocates for Lallemand's plan, that the nitrate will accomplish this work, and it is admitted that in some cases its action is perfectly effectual. But the operation with the *porte caustique* is extremely painful and also hazardous; it has not the merit of uniform success, and it does not supersede the necessity of other measures. It sometimes gives occasion for their most energetic use, for it is exceedingly apt to provoke a high degree of inflammation, and there is no calculating * * what final mischief it may do. The application of caustic gently employed, and not allowed to remain in contact with the parts for more than a second or two, may be a safe and justifiable procedure. It has been thus used many times without any unfortunate results. On the contrary, severe retention of urine, hæmorrhage, the most excruciating agony have been produced by the *porte caustique*. All these terrible effects may follow without one iota of benefit to the malady, for the cure of which the caustic is brought into the field of action. It is no more than truth to say, that Lallemand's instrument is now regarded with instinctive horror, and as a barbarous weapon; and its introduction into the urethra of any man should not be attempted until all other resources of surgery have been appealed to in vain."

Chapter XII. is devoted to a rare but extremely interesting form of disease, namely, gonorrhœal ophthalmia, "the true etiology of which is still the theme of discordant speculations and opinions. * * * Experience and common sense attribute the complaint chiefly to the inoculation of the poison derived from the urethra, or from an eye involved in the disease."

Contrary to the opinion of the author, we find no difficulty in "this idea," notwithstanding "that in the vast majority of instances in which the urethral pus is brought in contact with the eyes, no morbid action follows;" for the cases traced to this cause by honest and competent observers are positive evidence of a fact, and it is no more a requisite for the acceptance of the theory of inoculation or contagion in this disease that it should account for the failures, be they few or many, than it is indispensable, under the contagion theory in variola, to give full satisfaction as to why, out of a certain number of persons subjected equally to exposure, any should escape. It is enough, in a question of contagion, to advance positive testimony, against which negative evidence is feeble and inconclusive.

"Besides contagion, another mode by which blennorrhagic ophthalmia is believed to arise, is found in what is called *metastasis*, a term, the precise meaning of which is not fully agreed upon. * * * To me it seems that there is not sufficient evidence to sustain the theory of a metastatic form of this disease."

In this connection, the following passage from Desmarres, (*Maladies des Yeux*) may bear citation, in addition to extraneous support of which the author has availed himself:

"Saint Yoes, one of the first, spoke of metastasis, and his opinion, which many subsequent authors have adopted, was opposed by Scarpa; it is now abandoned in spite of the efforts of M. Boyer, fils. I have never witnessed the suppression of blennorrhagia when the eye is attacked. Chaussier, Tyrrell, M. Ricord report corroborative cases."

Chapter XIV. treats of *gonorrhœal rheumatism*, upon the cause of which, says the author, and, we may add, the precise

nature, "medical science has not as yet shed any light." Under the next caption we find *vegetations*, which "morbid excrescences were once, but are not now, regarded as the product of the syphilitic poison;" "they are often known under the name of acuminated condylomata, and have been very properly termed *papillomata*." And finally, a chapter on "Blennorrhagia in the female," prefaced by a eulogy of the speculum, terminates the first portion of the "Treatise."

Syphilis has received from the author, whose work we are noticing, a very extended consideration; and, as he is a partizan for views concerning its communicability which are not entertained by a large number of authoritative writers upon this subject, we propose to state his creed in the very language of the text:

"The virus (of syphilis) has its best expression—its most distinct embodiment, as well as its earliest incarnation, in the chancre; and we must regard this lesion, with here and there an exception, as the starting point or focus from whence result all the malign and diversified influences and developments known to the medical man as syphilitic. We may consider the fact as established beyond all reasonable doubt, that in all the varieties of the disease there is but one true poison of syphilis, which poison produces different effects, according to the nature of the tissue and the peculiar idiosyncrasy of the constitution in which the disease is manifested; all these different phenomena depending on the same morbid cause." * * * "Thus, if one portion of a primary sore is situated on the corona glandis, and the other on the mucous surface of the prepuce, the induration will form at the base of the latter only."

Again:

"It is well known, that if several men have intercourse with a woman diseased with syphilis, they may not all exhibit the same symptoms, although all receive a venereal infection. Mr. Acton relates the following: Three students had connection with the same *grisette* during one evening. One was affected with a phagedenic sore; one with an indurated chancre; and the third had a simple excoriation, which was slighter than that which existed on the genital organs of the female, who was examined a few days after the debauch," &c.

And in a note, p. 176, we find it stated :

“It is plain, that if we admit the doctrine of duality * * we may do so without infringing upon the unicity of the *syphilitic* poison ; because, from all we have said, it would appear that there seems to be a duality of *chancreous* poison, but that there is but one actually syphilitic virus capable of infecting the economy.”

Now, while we raise no question as to the inoculability, communicability or contagion of chancre, and admit (as on p. 177) that “probably not one chancre in twenty possesses the characteristic induration of its base and edges, and all the other peculiar attributes, which constitute what is called the Hunterian chancre,” we cannot but regret that the author should have refrained from offering any explanation, positive or probable, of the acknowledged relative infrequency of the “classical chancre” in our own day. It is beyond question, if we credit authority, that Hunterian chancres not only bear a very small proportion to the entire number of primary syphilitic manifestations, but that proportion has suffered a constant diminution since the days of the great surgeon whose name has become adjectively coupled with these bites of the serpent. The matter has, not very remotely, been made the subject of serious discussion ; and among the agencies claimed to have influenced the existing numerical relation between indurated and other primary sores, the syphilitic virus itself stands first, impressing the system of its earliest or unpolluted victims, and of their offspring, to such an extent that a new graft of the disease, upon a stock more or less tainted, produces a fruit specifically, but not generically, different from the horrible original. Whatever consequence be attached to this theory, all observations undertaken with a view of unveiling the truth, and not for the express purpose of sustaining hypothesis, must prove a benefit to the medical profession and to the community ; and we feel that the author of the treatise which now occupies our attention, has been ungenerous towards his readers in withholding from them his convictions upon this point, to which his capacity and extended field of study should im-

part especial value. In alluding to this topic, however, we would not wish to be understood as giving countenance to the inoculation of syphilis, as by Percy, who "practised it with a view of modifying an obstinate or incurable affection of long-standing," nor to its inoculation, as by Auzias de Turenne, who proposed thereby to attain a point of saturation of the system, or syphilization. "Most certainly not," to employ the language of the author, who is, by no means, an advocate of Turenneism, "until investigations and experiment shall ripen into higher completeness, and the sanative power of the measure shall have had time to exert a more persistent influence than has yet been displayed in patients who have resorted to prophylactic syphilization."

And with a bitterness which many will applaud, he continues :

"Without entering into any discussion of the real truth or fallacy of the doctrines broached by the advocates of artificial inoculation, it is enough to say that the demoralizing associations and consequences connected with the practice, are sufficient to consign it to unqualified condemnation. The most revolting feature appertaining to syphilization, and the one calculated to provoke unmitigated indignation, consists in the bold, bare-faced suggestion, that it may be employed as a means of safety for persons who are as yet untainted, and who can subject themselves to this factitious disease, and ever after be shielded from an infection, however deeply they may plunge into the foul cess-pool of licentiousness and corruption."

We have given prominence to the chief cause, alleged by some, to have influenced the decreasing ratio of Hunterian chancre, as well for the interest which attaches to it in this connection, as for the bearing which it evidently has in respect of another question, namely, "secondary symptoms without primary." For it must be conceded that individuals, naturally or artificially contaminated with syphilitic disease, must be placed in the same category, as far as their capability of communicating the post primary symptoms is concerned, since there is no evidence of which we are in possession, that artificial syphilization, which, according to Turenne, protects the

subject of it against the consequences of libidinous exposure, precludes the possibility of "evil communication" by that subject, in the form of secondary manifestations, if these "accidents" are really communicable. It can hardly be claimed that artificially induced syphilis, any more than that acquired in the usual way, extinguishes itself in the victim upon whom it is fastened.

The citations that we have made from the author render it sufficiently clear, that he admits or approves of the theory of "secondary symptoms without primary;" and, in the twenty-sixth chapter (and in the appendix), a number of cases are given which are "calculated to shake one's faith in certain views promulgated by Ricord, that prince of syphilographers."

All cases of secondary syphilis, not having been preceded by the primary symptom, may be arranged in three natural groups: 1. Cases of hereditary syphilis, about which there exists no discrepancy of opinion. 2. Cases of secondary syphilis, consequent upon sexual intercourse, between a syphilized male, presenting no outward marks of constitutional taint, and a healthful female.

We do not propose to discuss the question that here arises upon the authority of Mr. Porter, and which has one advocate, at least, in our country; but prefer to reserve what remarks we have to make for—Group 3. Cases of secondary syphilis, originating by inoculation or contagion of secondary "accidents," or of the blood.

By far the strongest evidence adduced by the author is to be found under the caption, "Transmission of the virus from the nurse to the infant, and vice versa," in the appendix, and on page 278. All the cases, detailed in the parts of the book above indicated, are drawn up with great care, and seem to be conclusive as to the communicability of syphilitic contamination from infant to nurse, or from nurse to infant. The last case to which we have drawn attention, that on page 278, reported from the records of the Massachusetts Hospital, is invested with peculiar interest; for not only would it appear that the venereal disease, occurring in an infant affected with

hereditary syphilis, was transmitted to the nurse, but that two persons, a mother and daughter, who kindly undertook the task of relieving the over-distended breasts of the nurse after the sudden death of the nursling, were infected with lues from the sore nipples of the syphilized nurse.

It cannot be denied, that in such cases as those above referred to, testimony may be obtained which is not so open to incredulous criticism as in those instances, for example, in which the husband or wife betrays his or her partner with a kiss. Thus—Case 1, page 270—Mrs. A. was affected, three months after marriage, with syphilitic eruption, and this was followed by other secondary symptoms. “The husband was of a roving character, and it was known that he had the venereal complaint about the time of marrying this woman.” Now, although we are informed “that there was not a particle of evidence that this patient ever had primary symptoms in the organs of generation,” we are left in ignorance as to the symptoms presented by the “roving character” at the time he ceased to be a Benedict. We know that “primary venereal sores can be cured without mercury, and in a shorter time than when that mineral is used;” and, consequently, an unseen vaginal chancre might be healed by remedial applications prescribed for leucorrhœa. Indeed, it is not absurd to admit the spontaneous cure of the Ricordian *fons et origo*. In view of these facts, we must return the Scotch verdict, “not proven,” in the case of Mrs. A.

Case II. A young, unmarried man had an eruption which was considered, by Dr. Winslow Lewis, to be syphilitic. The patient “frankly admitted that he had been on intimate terms with sundry girls of the town, but had never, *to his knowledge*, had chancre or other trouble on the penis or in the groin, nor had he ever had any suspicious discharge or soreness.” The italics are our own.

Case III. “A frail young girl had had, for more than two years, an extensive crop of syphilitic pustules and ulcers.” She stated that she never, to her knowledge, had had primary

symptoms. She was free to answer any questions, and appeared to have no sense of shame or delicacy.

Case IV. "Mrs. H. had been troubled for six years with venereal symptoms in the throat, on the tongue and on the skin."—"The husband states that he had chancre one year before their marriage, which took place in 1828. Both parties continued in health until 1848, when secondary accidents displayed themselves upon the husband, and continued to trouble him for two or three years. He now has several subcutaneous syphilitic gummata on the right arm and elsewhere. In 1852, that is, *twenty-three years after her union with this man*, the woman first noticed any venereal affection, and this was secondary."

The point of singularity in this case is this, that, notwithstanding constant cohabitation, the "secondary accidents" continued to manifest themselves in the man for four years before the infection of the wife.

Case V. "Mrs. R. was sadly afflicted with constitutional syphilis. She reports that she has never experienced any morbid symptoms in the genito-urinary organs."

We have given a brief summary of the cases preceding as exemplifying the nature of the evidence relied upon by the author; and we are quite willing to grant that "they have not been introduced * * for mere effect, but for the purpose of presenting what I consider to be the facts in the history of syphilis, and which are important to be known by every practitioner who has any professional dealings with this strange disease." Without fear of being accused of caviling, we state our opinion, that four of these cases are not conclusive, because they are incomplete; and that Case III. brings the least conviction from the character of the witness who makes the statement. It is necessary in a case bearing upon the point at issue, that we should have positive knowledge as to the soundness of the entire genito-urinary apparatus of the female before and after her exposure to syphilitic contamination, and the condition of the equivalent parts in the male, be he hus-

band or lover. We also claim to be "aware that it sometimes requires no little tact to evoke the truth from the polluted recesses of the mind in syphilitic subjects;" but that tact must be assisted by favoring circumstances; and if the author have, in the matter of proof, fallen somewhat short of his expectation, we cannot lay the charge to his want of skill, but to the knotty nature of "this strange disease." We feel certain that he has not formed his conclusions altogether from the evidence afforded by the five cases he has adduced, although these are not without their value in supporting a theory which numbers among its advocates Velpeau, Sigmund and Hebra.

The chapters which follow upon cutaneous eruptions are extremely well conceived and executed; the opinions they embrace are sound and orthodox; and their perusal cannot fail to afford ample satisfaction. Indeed, the whole subject of constitutional syphilis in the adult and the infant is treated with ability, and little is left to be desired on the part of the reader.

As a whole, the treatise on gonorrhœa and syphilis cannot lay claim to great originality; but, in compensation, it is written in a clear and conspicuous style, the natural consequence of very extensive practical familiarity with the matters discussed; and, what is of high consequence, the most sincere honesty of purpose every where pervades the work. The volume is a very interesting one, and we feel assured that it will be consulted with advantage, both by student and practitioner.

We take pleasure in commending the highly creditable typographical execution of the book, and in testifying to the excellence of the chromo-lithographic plates.

C. J.

II. *Lectures on the Diseases of Infancy and Childhood.* By CHARLES WEST, M. D. Third American edition from the fourth London, revised and enlarged.

To occupy in medical literature, in regard to diseases of children, the enviable position which Dr. Watson's Treatise does on the diseases of adults, is now very generally assigned to our author, and his book is in the hands of the profession everywhere as an original work of great value. One of the strongest recommendations of this work is, that it was not a treatise written either to sell or to manufacture a reputation, but it is composed of lectures delivered as the fruitful results of the observations of a careful and well trained physician upon a large number of medical cases, amounting at the time of its first publication to nearly 14,000. Since then, Dr. West has revised and enlarged his work as his extensive experience corrected or modified his views. We have moreover in the volume before us several new lectures, never before given to the profession in this country, but which were presneted to the English public in the Medical Times and Gazette of London. We had hoped to have found another lecture of Dr. Wests, as there published, on the "mental derangements of children;" but the work itself contains so much material for thought and so much sound pathology for instruction, that we ought not to wish for more. May we express our gratification, notwithstanding the opposition of the distinguished Philadelphia professor and author to books of foreign manufacture, that Dr. West's work has been so extensively adopted as a text book. Whatever may be said politically of the question of free importations, it would indeed be a narrow, contracted view in scientific matters, that we are not to have the benefit, and our students also, of the very best works anywhere published. Instead of insisting upon undergraduates buying as text books the works of their Professors, who ought to give all their peculiar (if any?) views to them in their lectures, it is far preferable to re-publish for them the thoughts and observations of our English and French distinguished authors, even if we have the title page disfigured by an Ameri-

can name as editor, when for the sake of the copyright, the new publisher has insisted upon his adding his comments or some few passages in brackets, thereby strongly reminding us of the engraver who added a fringe to his picture of the comet to give it grace !

To those of our readers who possess any of the former editions of this work, we advise to get this new one, as they will find sufficient new material to justify the investment. Some of the chapters are very much enlarged, and contain the more recent views. We are glad to find that Dr. West gives with accuracy the differential diagnosis between scarlet fever and diphtheria, showing clearly the mistaken view advocated by some of their identity.

Like all the prominent pathologists of our day, Dr. West is not satisfied with inflammatory manifestations of disease, but he dives deeper and looks to the blood circulating throughout the whole system for the cause of the local troubles; thus he arrives at more correct ideas of the nature of the disease and clear indications for treatment. He very appropriately classifies diphtheria among the blood diseases, and states what is now becoming well known, that death is produced ordinarily, not by the local exudation, but by the depression accompanying it, but not consequent upon it. The important bearing this view of its pathology has upon the treatment will instantly occur to our readers. Are we ever justifiable, with such a poison at work depressing the vital powers, to deprive the blood of its vivifying properties by lessening the amount of red globules ? And moreover, do we not in this way, as in rheumatism, increase the probable deposite of lymph ? Even leeches to the throat are of very questionable propriety, for the swelling is not mere active congestion of a sthenic character, but sluggish in its nature, with serous exudations into the cellular tissue and fibrinous concretions outside on the surface of the mucous membranes. An important question arises in the treatment of diphtheria, whether or not the operation of tracheotomy ought to be resorted to in any case. We would venture to answer in the negative, because even in true membranous croup it ought

only to be performed when the patient is dying from mechanical obstruction to the free access of air to the lungs. Trousseau never operates in diphtheria.

This question has a sad interest to us, as one of our most distinguished co-editors recently lost his life (apparently, at least,) from the infection caught in operating on a poor patient at a hospital, and, as has been stated, he himself insisted upon being operated upon when he found himself sinking from the same disease, and, although it gave him great temporary relief, yet it had no material influence upon his disease, as he sank a few hours after the operation.

Dr. West endorses the tonic and stimulant treatment of diphtheria, but prefers bark and mineral acids to the muriated tincture of iron. He insists upon the importance of the topical application of lunar caustic, or, what we consider still better, the commercial muriatic acid.

Perhaps the most important chapter of Dr. West's is the thirty-first, in which he treats of the peculiarities of the digestive organs of children, the composition of milk, &c., giving many admirable hints as to the hygienic management of children—rules which are far more valuable in restoring their deranged functions than all the drugs of our Pharmacopœia. If this lecture was only more read by practitioners, and itself well digested, we feel confident there would be yearly a much smaller number of these innocents sacrificed to heroic practice! It would be an addition even to the much good common sense about children found in that classical little book, known as "Combe on Infancy." Surely, if practitioners would stop to think of the indications nature has given us as to the proper nourishment of children, and the entire absence of starch in milk, we would not have them trying to force into their stomachs such, to them, utterly indigestible substances as pure starch in the shape of arrow-root or flour, (somewhat better, for it does contain some gluten,) biscuit-powder, and such compounds as druggists patent to sell for this express purpose. Dr. West then insists upon the use of *milk* in some shape or other, if not human, (always best,) yet cow's milk, goat's or

asses'. After deciding that the only substitute for the mother's milk is milk of some kind, Dr. West copies Vernon's and Becquerel's analyses of the different milks, showing that the asses' milk most closely resembles that of the human subject—the only difference being in rather a smaller proportion of oily particles. In Paris, where asses' milk is so readily obtained, the practitioners are in the habit of ordering it for children, brought up by the hand, but with us cow's milk must ordinarily be the substitute, and this must be adapted, and how this is to be done is a very nice and a very important point; and, beyond saying that it must be diluted, we find but little that is satisfactory in Dr. West's pages. We cordially wish, for the further instruction of American practitioners, that the writer had appended a synopsis of Dr. Cumming's paper "*on Natural and Artificial Lactation*,"* which contains many important directions, founded upon correct scientific data. How often are we puzzled by the mother's questions as to how much water is to be added, and whether or not the water will do no harm, and then must we not all acknowledge, that notwithstanding all our care in having the vessels thoroughly cleansed and scalded, and every precaution taken in having the milk from one cow, and sweet; yet that, in very many cases, the stomach of the child cannot digest it, and it passes through the digestive tube, irritating as it goes, and we have annoying diarrhoeas, or white casein stools, with the child's complexion looking as if it had been fed on chalk; whereas, if it was nourished properly, its color should be of a delicate fine rose tint and the alvine discharge of a light yellow, for it ought to be borne in mind that in milk there is no refuse matter—and whenever we find particles or lumps of white or green curd, it shows the food is not appropriated. Having for several years been very successful in regulating children's diet, and very often curing their summer complaints by following out Dr. Cumming's rules, we hope we will be excused, in this connection, for giving them to our readers in his own words, and begging that others may try them in their practice. The great diffi-

* American Journal of Medical Sciences for July, 1858. .

culty in preparing cow's milk for human babies' stomach is, that the difference is not merely in the casein, in which case simple dilution might be all that is necessary, but the other ingredients vary in proportion. Dr. Cumming says :

Cow's milk contains,

Butter	38.59
Casein	40.75
Sugar	53.97
Water	866.69—

While human milk contains,

Butter	20.76
Casein	14.34
Sugar	75.02
Water	889.88

“Cow's milk, therefore, contains nearly three times as much casein as human milk, but less than twice as much butter. In cow's milk, the butter is to the casein as 100 to 105; in human milk as 100 to 70. If then, by dilution, we reduce the butter to 20.76, we shall have 21.92 of casein, or 50 per cent. more than in human milk. With such an excess of casein we cannot hope to succeed.” “If, by a further dilution, we reduce the casein to 14.34, we have only 13.58 of butter, or less than two-thirds of the proper proportion. Such milk may, for a season, seem to suit the child, but before long it will be found that it does not thrive. The reason is plain. The right proportion of butter is 20.76; this warms a child, and supplies nervous energy. But, by withholding one-third, you lower the temperature of the body and deprive the nervous system of one-third of the special nerve-food, the indispensable *lecithin*. In a short time pallor and languor supervene, and health evidently declines, &c., &c.” “It is thus evident that by no mode of dilution can ordinary cow's milk be made a substitute for human. There will be, in every case, an excess of casein, or a deficiency of butter. So long as the butter is to the casein as 100 to 105, instead of as 100 to 70, so long must dilution fail to adapt it to the wants of the child. But if this original proportion could be changed to that existing in human milk, we might have hope of success. And we proceed to show how this may be done. If we leave at rest for four or five hours ordinary cow's milk, and then remove and examine the upper third, we find in it 50 per cent. more butter than it at first contained. In round numbers, its butter is no longer to its casein as 100 to 105, but as 150 to 105, or as 100 to 70. If then, by dilution of this milk, we reduce the butter to 20.76 we have 14.34 of casein, as in human milk. By using the latter half of the milk furnished by the cow, we have 54 of butter to 38 of casein, the right proportion exists, and by proper dilution it may be made to resemble in its chemical constitution human milk.”

We are tempted to quote the admirably practical suggestions of Dr. Cumming, but, to do it justice, we would have to insert the article bodily. For the information of those to whom it may not be accessible, we annex Dr. Cumming's schedule, where, in imitation of the natural process, he attempts, by the gradual diminution of water to adapt the food to the growing energy of the child. He states that it is better to begin with milk more diluted than the age and development would seem to indicate, and then gradually increase its strength.

DR. CUMMING'S SCHEDULE.

	<i>Milk.</i>	<i>Water.</i>	<i>Sugar.</i>
For a child from 3 to 10 days old,	1000	2643	243
For a child 1 month old,	1000	2250	204
For a child 2 months old,	1000	1850	172
For a child 5 months old,	1000	1000	104
For a child 6 months old,	1000	875	94
For a child 11 months old,	1000	625	73
For a child 18 months old,	1000	500	63

By using cow's milk thus modified, we have truly *artificial human milk*, on which children thrive, all their bodily functions performing with vigor and energy. We have, ourselves, seen such good results from this mode, that we believe Dr. C. is not exaggerating when he states that thousands of infantile lives might be saved by it.

In our large cities we have the further difficulty of the nourishing properties of milk having been seriously impaired by the animals being fed on swill, which renders the poor creatures themselves diseased, and then, by developing abnormally the alkaline salts interferes with the coagulability, and consequently with the digestibility of the milk.

We commend especially Dr. West's lectures on "Cerebral Symptoms" and on "Diarrhœa," as well calculated by their sound pathology to modify, in regard to the former, the application of blood-letting, and, to the latter, to correct the prevalent and incorrect notions that the liver is at fault, and the consequent abuse of mercurials in their treatment.

III. *The Institutes of Medicine.* By MARTYN PAINE, A. M., M. D., LL. D., Professor of the Institutes of Medicine and Materia Medica in the University of the City of New York; Corresponding Member of the Royal Verein für Heilkunde in Preussen, &c., &c., &c. Fifth Edition. Harper & Brothers, New York; Sampson Low, Son & Co., London. Pp. 1109, 8 vo.

THIS work of Dr. Paine is already well known to the scientific world, where it has met with much favor on the one hand, and much opposition on the other. Dr. Paine is a bold and uncompromising champion of the great theory of solidism or vitalism, against all other theories on the phenomena of life, whether chemical, mechanical, mathematical, or what not, singly, or in combination. Until we read this work, we thought the modern philosophers had it all their own way, and that the *vital principle* had been fairly, or foully, put to death; or rather, put out of sight, like a candle hid under a bushel. We have never yet believed in the extinction of this subtle principle, a "principle distinct from all other powers of nature," which, in the living being, has rule over them.

Dr. Paine tells us that the physiological world of the present day is divided into three schools: 1, the *Chemical school*; 2, the school of *Vitalism*; and 3, the school of *Chemico-vitalism*. The first "virtually regards organic nature as a part only of inorganic, endowed with the same properties and governed by the same laws." The second "regards organic and inorganic nature as distinct in their most essential attributes. It supposes that each department is governed by properties and laws peculiar to itself." "The third school, or that of *chemico-vitalism*, endeavors to form, as it were, a bond of union between the schools of pure vitalism and of pure chemistry."

The general suffrage of the Profession has been yielded, as our readers well know, for some years to the first and third schools; the second being quite in the shade, or, as many doubtless thought, quite abandoned. We read but the other

day, in a very clever work intended for the best informed portion of the public, that "the hypothesis of a Vital Principle, which was dominant for many centuries, is now rejected by all, except a few metaphysicians and metaphysiologists. It has fallen into complete discredit since men have learned that, besides being in opposition with all the teachings of science, it is really nothing more than a substitution of words for ideas, and while seeming to explain the phenomena, it only gives a verbal explanation, which leaves the problem unsolved. The same remark is true, although to a less extent, of the modern doctrine of a Vital Force or Vital Forces." * * * * "A vital principle is incapable of proof; if it exist, we cannot know it; and unless its existence can be proved, it is a mere phrase concealing our ignorance." (*"The Physiology of Common Life,"* by George Henry Lewes.) We do not propose to argue this matter with Mr. Lewes, but let us follow him for a few pages, to the DEFINITION OF LIFE. After criticizing various definitions, such as that of Bichât, "Life is the sum of the functions by which death is resisted," he gives us conclusively his own formula. "Perhaps the most intelligible and easily remembered definition would be this: '*Life is the dynamical condition of the Organism.*'" We fear this is but little better than "a phrase to conceal our ignorance."

Dr. Paine stands up manfully for the Vital Principle, and brings its most strenuous opponents as witnesses of its existence. Liebig, himself, the great chief of the chemical school, is made to acknowledge it. The phenomena of life, whatever part may be performed by chemical or mechanical action, are finally under its rule. In this investigation of life, there is a point where all the forms of philosophy find an insuperable barrier. Some, many difficulties, indeed, may be cleared away, but there is a barrier which will never be passed by human research or wisdom. Behind this, the Vital Principle sits enthroned. We do not comprehend it, we need not; we may approach it, perhaps, hereafter much more nearly than now, but it will never be subject to us. Avicenna attributed the coming on of labor to the will of God; Dr. Richard Mead,

in giving "a succinct idea of the nature of the human body," tells us of the numerous tubes that convey the blood and the 'several humors,' and, "in particular, that subtle and elastic fluid generated in the brain, and known by the name of animal spirits, the instrument of sense and motion." It takes a life-circle to keep up this active agent; "how can the heart contract to push the blood forward without the assistance of the animal spirits; or the spirits be secreted without the brain?" It hath pleased God to make of our body a "complete machine," which He alone could bring to perfection, and endow with that vitality competent to keep all in motion.

Surely the great Creator has implanted a Vital Principle in all that part of creation which has life; a principle which pertains to the secrets of Divine Wisdom, and which will exist by His will, whether we ignore it, or call it by another name.

"A VITAL or peculiar governing principle," says our author, "or power, in organic beings, has been recognized by all the most distinguished medical philosophers, at all ages of the science. It is the fundamental cause of growth, nutrition, and of all other phenomena of organic beings. It is, in all but the vulgar acceptation, synonymous with the term *life*; and life, therefore, is a *cause*, and not an *effect*, as has been assumed by many distinguished physiologists."

The Greeks, with their keen intuitive perceptions, saw in life both cause ($\psi\upsilon\chi\eta$) and effect, ($\xi\omega\eta$) and, indeed, it appears to have either relation, according to the point of view from which we observe it.

We are not prepared to go into any general review of Dr. Paine's work. We have only attempted to indicate the foundation upon which he builds his system of medical philosophy. We will say that throughout he sustains himself with great ingenuity and with great ability. If his arguments do not always carry conviction, they are at least always well put and forcible. His work manifests great learning, great reflection, and perfect independence of thought. Upon many points, we think he overthrows the theories of his adversaries, and when he does less, he gives such blows as shake them from top to base.

For ourselves, we frequently differ from him, and find him upon many points unsatisfactory, but withal we believe he holds the clew to many truths not held in the rival schools. In our anxious search for truth we are willing to take such grains of it as may be found in any quarter, while we hold ourselves

“Nullius addictus in verba jurare majestri.”

The great problem remains yet to be solved.

When we say, summarily, we often differ from Dr. Paine, we would be led into an elaboration of argument, beyond our present purpose, were we to attempt to canvass them.

A single brief paragraph, such as the following, might afford a text for an argument of interminable length :

“In the medical and physiological commentaries, I have set forth a variety of other important facts to show that motion, voluntary as well as involuntary, is essentially independent of the nervous system. The experiments of Philip are also conclusive upon this subject.” (P. 128.)

What shall we say of such a proposition as the above? Only this, that in animal life the nervous system is the machinery by the aid of which motion is effected. The author says upon a preceding page (126): “Voluntary motion proceeds from the action of the will upon the great nervous centre, by which the nervous power is developed and transmitted to the irritability of the voluntary muscles.” And elsewhere, (page 127): “It is now important to repeat that the nervous power never generates motion, *per se*.” Let us grant that the nervous power does not generate motion *per se*, does this prove that motion is essentially independent of the nervous system? We think not. Galvanism and electricity may have an independent existence, as indeed they have, but we cannot use them without machinery. The electric telegraph depends upon batteries and wires, which are essential to its operations, and in the same way we should say, that whatever may be said of mobility or irritability in the animal tissues, and however it may be said that the nervous system can effect nothing without them, yet that motion cannot be independent entirely of this machinery.

The illustrations to the contrary from vegetable life, and *apparent* illustrations from living animal matter, cannot be deemed conclusive in favor of the author's unconditional proposition.

We admit that we cannot here do justice to his argument, nor can we elaborate our own. Our object is simply to show in this specimen why we deal rather in some general than in any specific objections to his always well argued propositions.

It may not be considered amiss to conclude this notice with the following appropriate passage, which fell under our eyes in another line of study. Supposing we were going very deep in the sciences, we would learn, as a modern publicist says :

“Que nosotras somos un arcano á nuestras propias ojos y que hasta ahora todos los esfueryos de la ciencia han sido impotentes para explicar los fenómenos que constituyen nuestra vida, que nos hacen senter nuestra existencia; conocieran que el ruas precioso fruto que se recoje en las regiones filosoficas ruas elevadas es una profunda convicción de nuestra debilidad é ignorancia.” (Balmes.)

(We would know that we are a mystery to ourselves, and that to this day all the efforts of science have failed to explain the phenomena which constitute our life, and make us conscious of our existence; we would know that the most precious fruit gathered in the highest regions of philosophy is a profound conviction of our own weakness and ignorance.)

Here, at least, is sober truth, which will probably be truth to the end.

R. McS.

CHRONICLE OF MEDICAL SCIENCE.

MEDICAL PATHOLOGY AND THERAPEUTICS.

1. *Discussion on the Therapeutic Value of Perchloride of Iron before the Imperial Academy of Medicine—Speech of M. Trousseau.*

Mr. Devergie, in the name of the Committee, of which he was a member with Messrs. Bouchardat and Bouillaud, reported on a memoir by Mr. Pize, a medical practitioner of Montélimart (Department of La Drôme) and entitled: *De l'emploi de perchlorure de fer dans le traitement du purpura hæmorrhagica et de son action sédative sur le cœur* (On the use of sesqui-chloride of iron in the treatment of *purpura hæmorrhagica* and its sedative action on the heart.) This paper is divided into two entirely distinct parts; one relating to the exposition of practical facts; the other to the mode of action of sesqui-chloride of iron on the system in disease.

The following are the obvious inferences from the cases relating to *purpura hæmorrhagica*:

1. Sesqui-chloride of iron is pre-eminently the agent for the cure of the disease; it arrests the hemorrhagic tendency in the space of twenty-four or forty-eight hours, and, continued for a few days, rapidly brings about the convalescence of the patient.

2. This medicine produces an immediate diminution in the rapidity of the circulation, decreases the quickness of the pulse in twenty-four hours from 110 to 80 pulsations, and may therefore fairly be considered as a direct sedative of the action of the heart.

M. Trousseau addressed himself to both the questions of fact and of theory. The four cases, which form the basis of Mr. Devergie's report, although relative to very serious instances of *purpura hæmorrhagica*, seemed to him insufficient to justify inferences so exclusive as those brought forward by

Mr. Pize. In this respect Mr. Trousseau participated in the opinion expressed in Committee by Mr. Bouillaud. This reserve and these doubts are more especially conceivable, as sesqui-chloride of iron failed in Mr. Devergie's hands in two cases of febrile *purpura* with successive eruptions. The lowering of the pulse after four or five days is usual in the natural course of febrile *purpura hæmorrhagica*. The diminished activity of the vascular system is therefore a spontaneous phenomenon, of which sesqui-chloride of iron does not deserve the credit. If, however, this remedial agent were a vascular sedative equal to digitalis and aconite, it would, like these substances, display this peculiar power in healthy subjects, a circumstance which occurs neither in man nor in animals.

Mr. Trousseau was of opinion that Mr. Devergie was wrong in dividing physicians into two camps, as to the interpretation of the therapeutic action of medicines. For most remedies, all are agreed; thus, none pretend to explain the efficacy of opium, belladonna, nux vomica, etc., on chemical grounds. If differences of opinion still exist with regard to a small number of substances, iron, in particular, it is unnecessary to make a distinction between vitalists on the one hand, and dynamists on the other.

Is sesqui-chloride of iron a hemostatic? It is and it is not. It is a *direct* hemostatic and one of the most energetic; Pravaz's experiments and the daily experience of medical practitioners abundantly prove it.

But is it an indirect hemostatic? The Professor did not think so; he argued that in uterine hemorrhage, for instance, it was inadmissible to suppose that sesqui-chloride of iron would successively traverse the capillaries of the alimentary duct, of the liver, of the lungs, etc., and pass through the greater part of the vascular system without coagulating one drop of blood, without producing the least hemostatic effect, and precisely exercise all its astringent and coagulating power on the capillaries of the womb! It is difficult to understand so strange a phenomenon! Mr. Trousseau did not, however, contest it; but he feared that it would be received with incredulity, even by the warmest friends of chemistry among his colleagues.

The restorative action of sesqui-chloride of iron has been also much exaggerated. Mr. Trousseau estimates that this salt, in this respect, is very inferior to other ferruginous preparations; it has, in addition, the disadvantage of not being easily managed and of being tolerated in general with difficulty.

Here the learned Academician raised the difficult and still very obscure question of the mode of action of iron as a restorative. It had been long believed and taught that iron, as an ingredient of the blood, was much decreased in quantity in chlorotic subjects. Recent experiments, instituted by Messrs. Favre and Reveil, have demonstrated that the contrary is the case. Thus, these skillful chemists have found that in the chlorotic, the amount of globules of the blood being represented by 40, the proportion in weight of normal iron is equal to that of a non-chlorotic subject, in whom the figure of the globules rises to 120 or 130, according to the investigations of Messrs. Andral and Gavarret. If, therefore, for the same quantity of blood the same proportions of iron are found in chlorosis, and when that disease does not exist, although the blood in the latter case contains three times more globules than in the former, it must be admitted that the iron is condensed in the globules of the blood of chlorotic subjects.

Mr. Trousseau admits the passage of iron into the blood, but hence it does not by any means follow that it remains in that fluid and that it becomes assimilated to the system. No substance is assimilated by force. If albumen be injected into the blood of an animal, it is eliminated by the kidneys. Sugar given in excess or injected into the vessels, also, instead of being assimilated, passes into the urine.

Thus iron penetrates into the blood, but does not sojourn there; the quantity absorbed is inappreciable, according to the experiments of Mr. Natalis Guillot, who has constantly detected in the fæces almost all the iron ingested into the stomach.

Therefore, although iron is of incontrovertible utility in the treatment of chlorosis, the mode of action of this medicine has not yet been discovered any more than that of the other agents of the *materia medica*.

Two illustrious chemists, Liebig and Dumas, have attributed to chemistry an exaggerated part in therapeutics. Chemistry must not direct, but merely enlighten medicine. And yet, God knows, if it is so! Mr. Garrod publishes a work on gout, in which he professes that this disease depends on an excess of urate of soda in the blood. Hence the chemical treatment so well known; hence those innumerable drugs, colchicum, Boubée's syrup, Lartigue's pills, Laville's remedy, etc., which have killed as many gouty subjects as the waters of Carlsbad and Vichy.

In this respect, the orator observed that the waters the most highly extolled for the lithic diathesis, Vichy, Carlsbad,

Pougues, Contrexéville, produce effects varying in inverse proportion to their alkalinity. Nothing is, however, more common than cures obtained by waters containing different mineralizing ingredients, or even containing none whatever, such as, for instance, those of Plombières and Bagnères-de-Bigorre, which are scarcely more mineralized than river-water. How can a purely chemical theory account for the fact that a patient, after a season at Vichy or at Pougues, remains a year without ejecting any calculi? Will it be argued that the lithic acid has been neutralized by the alkaline virtues of these waters? But obviously this alkali has long been expelled from the blood! If concretions ceased to be formed, it is merely because the constitution has been replaced in a more healthy condition.

Dyspepsia attended with acidity is cured by the use of the alkaline waters of Vichy, Carlsbad, Vals. But when patients find it more convenient to repair to Bagnères or Plombières, they at times recover quite as rapidly or even more so. Here again, therefore, if the alkaline medication is beneficial, it is not from its *alkalinity*. Moreover, Mr. Claude Bernard has demonstrated that, if an alkaline salt is given to a dog, bearing a fistulous opening in the stomach, the salt, it is true, instantly neutralizes the gastric juice; but at the same time a more abundant secretion of that juice takes place, so that the surest means of filling the stomach with acid fluids would, perhaps, be to exhibit an alkaline preparation.

Why is uterine hæmorrhage checked by cold affusions? Why are the catamenia suppressed after a glass of cold water has been taken into the stomach? We know not. Can we say we possess more accurate information on the subject of the unquestionable efficacy of the water cure, or do we know why metallic armatures, applied to a limb, increase ten-fold its muscular power in the space of half a minute? Why the irritation produced on the gastric mucous membrane by the contact of ipecacuanha, of tartar emetic, or of sulphate of copper, throws into convulsion all the respiratory muscles and induces emesis? Can chemistry supply us with an explanation of these phenomena? Why is it that waltzing, swinging, or the rolling of a ship bring on vomiting and vertigo? Why protracted tickling of the soles of the feet may cause death? Any physical or chemical explanations of these phenomena are untenable; far better is it to confess our ignorance.

"I am reproached," said Mr. Trousseau, "with always demolishing and never constructing." Granted; but I declare my utter inability to supply the required explanations.

"I am asked whether I am a vitalist or an organisist. I do

not know; I am, perhaps, both.... Instead of discussing these grave and insoluble questions, we should act more wisely were we to attach ourselves, in the first place, to ascertaining facts. ... In therapeutics, experiment must be the starting-post; systematization follows. Disease was first empirically cured; this has been the origin of the most active medications, and of those reputed the most rational. Before establishing the substitutive medication, irritant collyria were empirically introduced into the inflamed eye; before goitre and tertiary syphilis were treated by iodide of potassium, they were empirically cured with burnt sponge. Let us not be more ambitious, and our therapeutics will be sound.

“I recapitulate and say: Therapeutics will be nearer to the truth in proportion to the candor with which we shall agree to confess our ignorance as to the intimate mode of action of remedies; in proportion as we shall study more specially each medicine, and more closely apply ourselves to experiment. This does not exclude spontaneity or the primitive direction of experiments, which we should conduct and not permit to conduct us; nor does it exclude sagacity in research or philosophical deduction.”

2. *Remarks on Diabetes Mellitus.* By Dr. ARAN, Clinical Professor Hôtel Dieu. (Champonniere.)

In Saint Antoine ward, we noticed a woman, aged forty-six, who, eight months ago, observed on her linen and upon her shoes whitish stains, in which Dr. Béhier, a physician of the hospitals of Paris, detected the presence of diabetic sugar. In this case, the disease had originated twenty-two months previously. At that period the patient experienced a burning thirst, particularly at night. She was in the habit of drinking as much as fifteen quarts of water in the course of the day, and a quantity of urine, at least equivalent, was passed in the 24 hours, a fact illustrative of Thénard's remark, that in diabetes the quantity of urine secreted by the kidneys is generally superior to that of the fluids ingested into the stomach. The patient, moreover, had lost all taste for food, as it most commonly happens, although the contrary has been asserted; under the influence of the treatment instituted by Mr. Béhier, the appetite had improved, and she was in other respects progressing satisfactorily, when she became affected with cough, expectoration of greyish sputa, oppression and præcordial anguish extending to the throat, but without the propagation of pain to the left arm, peculiar to angina pectoris. On auscul-

tation, the signs of incipient pulmonary tuberculosis were detected, a complication frequently met with in diabetes. The thoracic symptoms, for which the patient had been admitted into the Hôtel-Dieu, speedily yielded to appropriate measures, diabetes remaining to be contended with, and towards this complaint the attention of the Professor was exclusively directed.

Other signs, in addition to excessive thirst and polyuria, point to the probable existence of diabetes mellitus. We may mention, for instance, the acid condition of the salivary secretion, a circumstance noticed by Mr. Mialhe, and the significance of which was fully confirmed in the present instance. On one occasion, by this single symptom, Mr. Mialhe was led to the discovery of diabetes in a person who was entirely ignorant of his condition. The best guide of diagnosis, however, is the examination of the urine. In health, this secretion presents a specific gravity varying between 1017 and 1020, and when its weight is found to exceed 1040, the presence of glucose may be strongly suspected. In the case alluded to above, the urinometer marked 1033.34. As to the actual detection of glucose, it is most commonly effected with Bareswill's solution, a very delicate and satisfactory test, when added to urine in the proportion of 2 to 1, and when the absence of albumen has previously been ascertained. In his patient, Mr. Aran found $9\frac{1}{2}$ drachms of sugar for 32 ounces of urine.

The functional disturbances arising from diabetes mellitus are well known, although both physicians and chemists remain in doubt as to their causes and seat. Mr. Bouchardat considers diabetes to originate in a morbid change in the digestion and assimilation of farinaceous substances, while Mr. Bernard refers it to a peculiar lesion of the nervous system. Mr. Alvaro Reynoso ascribes the disease to the imperfect combustion of glucose, from improper accomplishment of the respiratory functions; in Mr. Mialhe's opinion, diabetes is caused by the insufficiently alkaline condition of the fluids of the system, hence inadequate assimilation of glucose. Whatever be the respective value of these theories, Mr. Aran conceives that both Mr. Bouchardat and Mr. Mialhe have rendered signal service to the treatment of the malady, the former in pointing out the injurious effects of farinaceous nutriment, the latter in recommending the use of alkaline remedies. The history of the patient who suggested the present remarks, is confirmative of Mr. Bouchardat's theory. She chiefly fed on farinaceous substances; after consulting Mr. Béhier, she restricted herself almost entirely to azotized food, and, under the influence of this change, the amount of urine excreted fell from 15 to 11

quarts, and subsequently after a spontaneous attack of diarrhœa, by which this quantity was further reduced to 6 and even to 5 quarts in the day; she also derived considerable benefit from repeated doses of aperient medicine, and after four months' treatment she seemed restored to health. She then, however, returned to her previous regimen, and diabetes has re-appeared in consequence of indulgence in the forbidden articles of diet.

Diabetes has ceased to be a serious malady. For the last twenty years, Mr. Mialhe has attended patients affected with glucosuria, who may still continue to live long. It is, however, a cause of emaciation. Mr. Aran's patient, while in health, weighed a little above 15 st., and now she weighs but $9\frac{1}{4}$ st. On a former occasion, when descanting on the utility of the ophthalmoscope, we mentioned the disturbances of vision, to which diabetes gives rise. It is, therefore, a complaint which should be treated, and although Mr. Aran is inclined to think it is not one which can be radically cured, still he doubts not that diabetic subjects, provided their constitution has not been too deeply injured, may, by proper care, ward off its fatal tendencies.

For this purpose, the action of the skin must be solicited by violent exercise, vapor baths, and flannel garments. The food should be highly azotized, and bicarbonate of soda be exhibited in accordance with Mr. Mialhe's views. Mr. Aran does not venture on any rational explanation of the *modus operandi* of this system of treatment: the chemical data on which it rests may be correct or incorrect, but the results are manifest, and it is not without reason that Vichy is crowded with patients afflicted with diabetes. In the case of the patient lying at present in the wards of the Hôtel-Dieu, the daily prescription is the following:

One ounce of bicarbonate of soda to be taken in the drinks.

Claret, 16 ounces.

Bark-wine $3\frac{1}{2}$ ounces.

Full diet of meat, equivalent to about 24 or 26 ounces of animal food.

A vapor bath, followed by cold affusion.

Spa-water at meals.

Four quarts of the usual diet drink.

THERAPEUTICAL RECORD.

Lithate of Quinine, a New Soluble Febrifuge.—

Popular remedies have occasionally been the origin of useful scientific discoveries: it need, therefore, occasion no surprise, that the cure of certain obstinate intermittent fevers, in patients who had swallowed their own urine, should have suggested to Dr. Perraire, of Bordeaux, the idea of exhibiting urea as a febrifuge, or, by a more felicitous application, of compounding a new soluble anti-periodic remedy by the association of uric acid with quina.

A paper published in the *Moniteur des Sciences Médicales* by Dr. Armand de Fleury on urate of quinine, shows that salt consists of ten parts, in weight, of amorphous quina, and twenty of pure crystalized uric acid. It is prepared as follows: 16 ounces of distilled water are poured into a retort which can stand the fire; the trade amorphous quina is added during ebullition, which is kept up for ten minutes; the crystals of pure lithic acid are then thrown in gradually, the mixture being at the same time stirred with a spoon. It is allowed to boil for one hour, a quantity of distilled water being added from time to time, sufficient to supply the loss of the vaporized fluid, and the preparation is then filtered and decanted. The residue is boiled anew for twenty minutes, with an amount of distilled water equal to that used during the first process, passed through the same filter, and the two strained liquids are mixed and evaporated over a slow fire until complete desiccation. A salt is thus obtained, of a fine yellow color, occasionally amorphous, but usually in very bright crystals. Lithate of quinine dissolves during ebullition, or at even a lesser heat, and more sparingly in cold distilled water.

Such is the nature of the salt, which its inventor, and, at his instigation, numerous French and American practitioners have substituted with advantage for sulphate of quinine, not only in the treatment of ague, but in all intermittent forms of neuralgia or neurosis, in short in all cases requiring anti-periodic medicines. Not only is urate of quinine represented as possessed of a genuine and lasting beneficial action, but it is also reported not to cause the notable disturbance of the circulation and of the nervous system observable after large doses of sulphate of quinine.

Trichiasis ; Curling the Eye-Lashes.—In the first stage of trichiasis, the most rational mode of treatment, that which in general yields the most permanent results, consists in restoring the deviated lashes to their proper direction ; but the remedies recommended for the purpose, such as gum, collodion, lacker varnish, etc., are obviously insufficient to effect this purpose. Mr. Anagoskis, of Athens, proposes another, curling, the description of which procedure we borrow from the *Annales d'oculistique*.

“In order to curl the lashes,” says the Athenian surgeon, “I have caused an instrument to be constructed on the same principle as hair-dresser’s curling irons. One of the blades is cylindrical, and is received into a groove formed by the other, the two branches being kept apart by a spring. The eye should be protected by a piece of damp paper, slit in the middle ; through this aperture all the lashes are passed ; with the curling-iron, heated to a proper degree, they should then be curled upwards. This innocuous operation must be repeated from time to time, until the deviated hairs have resumed their wonted direction. I have frequently used this method with benefit, and although requiring some expertness, the curling of the eye-lashes induces no feeling of alarm in the patients, and is not objected to by the vainest or most timid subjects.”

Anduran Wine for Gout and Rheumatism.—

Corm. colchici,	. . .	1 ounce.
Fol. fraxini,	. . .	1 ounce.
Vini Albi (Malaga),	. . .	10 ounces.

Macerate for one week ; strain ; add

Tinct. aconiti,	. . .	2 drachms.
Tinct. digitalis,	. . .	1 drachm.

A tea-spoonful morning and evening in a cup of tea.—*Bouchardat*.

Chlorosis, Saint-Ignatius’ Bean alone or in combination with Chalybeates.—In a paper published in the *Bulletin de Thérapeutique* we observe that Dr. Eisenmann, of Würzburg, considering chlorosis as a nervine disease of which diminution of the healthy amount of blood-corpuscles is the usual consequence, but not by any means the indispensable element, endeavors to point out the necessity of opposing to the malady other remedies besides the trite exhibition of chalybeates.

Mr. Eisenmann and other physicians who adopt his views begin the medication of chlorosis by administering 10 or 15

drops of the tincture of St. Ignatius' bean morning and evening; and when the stomach has become used to the drug, it is combined with iron and rhubarb as follows:

- R.* Strychnos Ignatiæ, 1 gr.
 Ferri lactatis, seu limaturæ, . . . 4½ gr.
 Rhei, 3 to 4 gr.
 Olea-sacchari menth-pip, 6 gr.
M. To be taken twice daily.

Fluid Stimulating Glycerine Poultice.—In the same Journal, Mr. Grimault publishes the formula of a very simple and inexpensive combination, which will always supply a ready substitute for those most useful revulsive appliances, mustard-poultices. These require, however, a certain provision of mustard flour, warm water, a compress, and moreover the mustard not unfrequently deteriorates by keeping, and in long sea-voyages or distant expeditions, the physician may find himself unable to prepare this necessary and simple remedy at the time he most urgently requires it. Dr. Grimault therefore proposes to substitute for sinapisms the following mixture:

- R.* Glycerine, 3 dr.
 Amyli, 2½ dr.
 Ol. ess. sinaplis, . . . 10 minims.

A thin layer of this preparation spread over a piece of linen or sized paper promptly produces as powerful and as speedy a revulsion as could be effected by the best mustard.

If the glycerine is of good quality, the volatile oil of mustard can lose none of its strength. It is recommended to shake the mixture before using it.

Ferruginous Sugar-Plums containing Bismuth and Manna.
 An Orleans chemist, Mr. L. Foucher, in order to counteract the most usual disadvantages of chalybeates, has compounded a preparation which the *Journal de chimie médicale* describes as follows:

- R.* Ferri pyro-phosphatis, }
 Bismuthi trisnitratis, } a 1 g.
 Mannæ lachrym, 4 gr.

For one sugar-plum.

The active ingredient here is a fixed and readily soluble salt. The bismuth is destined to soothe the irritability of the stomach, and the manna to secure the permanent state of solution of the pyrophosphate of iron, while it may also obviate the constipation, usually produced by martial preparations.

EDITORIAL AND MISCELLANEOUS.

DR. HOLMES vs. THE MEDICAL PROFESSION.

The Anniversary Meeting of the Massachusetts Medical Society always produces a great stir among the fraternity of the Bay State, and many a hard-worked poor fellow whiles away his long and chilly ride with pleasant expectations of the good time coming.

Never was this anticipation more generally felt than on the approach of the last festival, held in Boston during the sweet sunny days of May. Nothing like it before. The councillors met, and the censors conferred together. The anniversary chairman announced his programme, and all the profession was alive with excitement. They should hear an address from that funny Dr. OLIVER WENDELL HOLMES. What a treat! You'll laugh yourself to death! He is really afraid to be as funny as he can. And then such a jolly dinner in the cradle of liberty!

Now, it is a cardinal principle, in all born of Massachusetts, to think that Boston is *the* city, and Dr. Holmes the smartest little man therein. Said Doctor has told them so, and they all agree with him. Isn't it the hub of the solar system, and though good Americans generally when they die, go to Paris, every true blooded Yankee would be satisfied to spend his here and hereafter in Boston.

So, in they came from all directions, with their wives and rosy daughters, with their wonderful cases and tedious statistics, but all agog with the coming sport, until more than four hundred of the working men in medicine were collected in the hall of the Lowell Institute, to hear the last effusion of the Professor, Poet and Wit, who rules over the morning meal, sparkles after dinner with toast and epigram, and takes his tea (without toast) at the Mutual Admiration Society, of which he is a prominent member.

It so happens, however, that the Professor, though clever, is

a curious little man. He has a good opinion of himself. It's the only fixed principle he has got; for in all other things he glories in his free thinking. He must have a sensation, and has no idea of going over the road worn in many ruts by the slow coaches who preceded him. No, indeed; there must be some startling proposition to ruffle the surface of this complacent audience, and nobody can get up the steam quicker than our orator. If you want a new poem, he will give it to you in a minute, (sometimes a right good one.) Would you like a new religion? You can soon be made a member of "*The great American Church.*" Jerusalem and Rome are trampled into dust in the triumphant march of the "three hilled city." Here are the geological "tables of stone," to be read by the wiseacres of Cambridge, for Moses and his law hardly suit the times. What, though Paul the Apostle lies buried with the fathers, don't Parker and the Professor reign in his stead?

Well, to make a long story short, the Professor's imagination happened to take the wrong turn, and our poor friends who had been so long saving their little earnings for this eventful occasion, were the sufferers; for, to their unutterable astonishment, the leading man in their medical school—their selected orator—had turned king's evidence, and was prepared to shew to them and the world the pernicious consequences of their art. They calkulated upon a soothing, agreeable cataplasm, applied with the gentlest hand, but found themselves cantharadized and croton-oiled at a fearful rate.

"Hasn't a man the right to think what he pleases and say what he thinks" in this glorious land of free love, free religion, and free thinking? "Isn't this the very aim and end of our institutions." Dr. Holmes has taught you, gentlemen, what you know of physic. He has spent your money and signed your sheep-skin. Who has a better right than he to speak his mind freely?

You need not pass a resolution "that this Society disclaims all responsibility for the sentiments contained in this address." The Autocrat is still in demand; the seventeenth edition of the poems is in press, and every body buys the Professor at the Breakfast Table, although the M. D. to the author's name is

carefully withdrawn from the title page. Sit still, my friends, and don't make an unnecessary to-do. If he "should strain the truth for the sake of an epigram or an antithesis," can't a man lie a little, rather than spoil a joke?

"My friends and brothers in art," you seem to be satisfied that you are practising a useful calling, which cures some, palliates much, prevents a good deal, and comforts all who require its aid. We told you so when at college, but now it is well to open your eyes to the truth. You are just helping those poor devils along a little faster by stuffing down their throats a number of noxious drugs. "If the whole *materia medica*, as now used, could be thrown into the bottom of the sea, it would be all the better for mankind and all the worse for the fishes."

We can make an exception of opium, "which the Creator seems to prescribe, as we often see the scarlet poppy growing in the corn fields;" but all the other recipes of Omniscience must be condemned. The purple fox-glove, the many-tinted veratrum, the lilac stramonium, they are all "*noxious*;" but a little opium—it helps the imagination.

Wine must be preserved. [You don't catch as knowing a fellow emptying his demijohns into Charles river.] "Wine is food," and "when a circle of clever fellows meet together to dine and have a good time of it," wine is "the great equalizer and fraternizer, which works up the radiators to their maximum radiation, and the absorbents to their maximum receptivity."

Don't throw away "the specifics." [You need not look self-satisfied, my good man, you didn't discover one of them. The Jesuits and the quacks and other people told us all about them, and we know how they should be used without your advice.] But with wine, comes the gout, and besides, I might get the chills and fever whilst peddling my tin cups about the country, (cheap article at fifty dollars a piece, and with more sawder in them than I intend *you* shall have to-day.)

Chloroform is worth keeping, or, in more elegant phrase, "the miracle of anæsthesia,"—as much a miracle as many, that old fogies, strapped down by thread-bare doctrines and antiquated creeds, think so much of. Away with all the rest.

[There is no mistake, but the Professor did give his friends and brothers in art a handsome drubbing.] Don't you know "that every noxious agent, including medicines, which would hurt a well man, will hurt a sick one." They did *not* know it; one pig-headed old grumbler, with a wide-brimmed white hat and a hickory cane, had thought that disease and health are opposites, and what's a well man's poison *might* be a sick one's meat. Pshaw, says the Professor of Anatomy in Harvard, the best system of practice that I know is the one "nature taught to the first mother, when she saw her first-born child putting some ugly pebble or lurid berry into its mouth, I know not in what language it was spoken, but I know that in English it would sound thus: *Spit it out.*"

[At this last shot, old Broadbrim gave an awful growl, and I know not in what language it was spoken, but I *think* that in English it would have sounded thus: PUT HIM OUT.]

Mixing mirth and sentiment with "practical hints," the orator treated his audience to a few clinical remarks, worthy of Graves and Bennet, which ought to be preserved without mutilation :

"One practical hint may not be out of place here. It seems to be sometimes forgotten by those who must know the fact, that the tongue is very different, anatomically and physiologically, from the stomach. Its condition does not, in the least, imply that of the stomach, which is a very different structure, covered with a different kind of epithelium, and furnished with entirely different secretions. A silver-smith will, for a dollar, make a small *hoe*, of solid silver, which will last for centuries, and will give a patient more comfort, used for the removal of the accumulated epithelium and fungus growths which constitute the 'fur,' than many a prescription with a split-footed R before it, addressed to the parts out of reach."

[There were many quiet, but attentive auditors of this pronunciamento of the funny doctor, who were fresh from the fight with gastritis, dyspepsia and typhoid fever. They would not appreciate the Professor's anatomy, physiology, or bedside knowledge.]

No, sir. Scrape their tongues. Massasoit had typhoid fever and Governor Winslow scraped his tongue. The result was,

that the grateful Indian, overwhelmed with the success of the practice, revealed a dark and murderous plot, and so preserved you all to this day. Yes, sir, it "may save the Union, some of these times, if a Presidential candidate should be taken sick and his tongue wanted cleaning."

[Suppose you get in practice on those foul-mouthed fanatics and blasphemers of God and the Bible, who enjoy "the large license of speech in the thinking centre of this continent, the brain of the new world."]

Here is another spicy paragraph which will amuse our readers "down in the provinces." They will be surprised if the graduates of Harvard, with the Professor to teach them clinical medicine, should be guilty of such blunders. Give it to them well; hilarious Blue-beard, tickle 'em to death:

"Again, see how the 'bilious' theory works in every-day life here and now, illustrated by a case from actual life. A youthful practitioner, whose last molars have not been a great while cut, meets an experienced and noted physician in consultation. This is the case: A slender, lymphatic young woman is suckling two lusty twins, the intervals of suction being occupied on her part with palpitations, headaches, giddiness, throbbing in the head, and various nervous symptoms, her cheeks meantime getting bloodless, and her strength running away in company with her milk. The old experienced physician, seeing the yellowish waxy look which is common in anæmic patients, considers it a 'bilious' case, and is for giving a rousing emetic. Of course, he has to be wheedled out of this, a recipe is written for beef-steaks and porter, the twins are ignominiously expelled from the anæmic bosom, and forced to take prematurely to the bottle, and this prolific mother is saved for future usefulness in the line of maternity."

Suppose, however, that all these noxious agents were good for the diseases for which they are recommended, still I would say: Throw physic to the dogs. What, do you want to cure that wretch "with tabes mesenterica, hydrocephalus, and other similar maladies—*the race would be ruined, if art could ever learn to preserve the individuals subject to them?*"

[Leave them in the wilderness says the savage, or the lonely cave. LET THEM DIE, responds the sentimentalist, poet, sage

and physician, who greases the axle round which rolls the hub of the solar system.]

Going about the country, continues the excited humanitarian, trying to cure poor women of their pains and aches. "*They ought to have head-aches and back-aches and stomach-aches ; they are not well if they do not have them.*"

[A trembling culprit shrinking under the lash—for he had been up all night with a sweet woman, mother of children, gentle of heart, who had one of the forbidden pains—exclaimed: Most learned orator, did you *ever* try to ease the pangs of sickness, to smooth the couch of suffering?

Bless me, was the ready reply, wasn't I once the physician to the Broad Street District of the Boston Dispensary, "and if any body got well under my care, it must have been in virtue of the rough and tumble constitution which emerges from the struggle for life in the street-gutters, rather than by the aid of my prescriptions."]

'Tis a pity that we have no room for more extracts from the Annual Address, and must imagine how the four hundred enjoyed their dinner after this traitorous onslaught from one who, honored and trusted, sat in the highest seat of the temple of medicine and tried to deface its fair proportions. But, if the friends and brothers couldn't laugh, all the homeo-quacks, herb-doctors and eclectics enjoyed the joke hugely; the penny-a-liners quote the professor in every newspaper, and advertisements introduce their vegetable remedies under the patronage of his high authority.

[Would some of our friends please send us a copy of the Professor's letter resigning his chair in the Medical School of Harvard? It should be placed upon record as a noble sacrifice of self on the altar of principle. He can never sign his name to the diploma of another "*Vir, ingenio bono, ac scientia utili præditus, moribus que probis ornatus, post tempus usitatem medicinæ studio et praxi impensum.*" No—no; rather, at the behests of a higher law, see him and his gallant followers as they riddle the accursed dens of Metcalfe or Chapman, and in glorious emulation of ancestral deeds, sending gallipots and pill-boxes to the bottom of Boston Harbor.]

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ART. I.—*A Review of the "Malarial Pneumonia" Controversy*, between Doctors Manson and Howard, with some remarks upon the Change-of-Type Theory of Disease. By F. DONALDSON, M. D., Baltimore.

THIS controversy consists of a series of articles recently published in the North Carolina Medical Journal, and originally read before the Medical Society of North Carolina, by Dr. Wm. T. Howard, in answer to a paper by Dr. O. F. Manson, written the year previous for the same Society. In order to do justice to both sides and review them impartially, we referred back to Dr. Manson's interesting articles as published in this Journal in September and October, 1857, and read them with care, previous to going over with attention Dr. Howard's able, but lengthy review.

Before entering upon the merits of this controversy, we may be permitted, with all who take a deep interest in medical progress, to express our pleasure at finding so much vitality in a State Medical Association; which, we learn, is a thorough State organization, meeting annually. With such medical papers as these to claim the attention of its members, the Society's influence throughout the State in advancing the best interests of the profession must be, ere long, very decided. This is a dis-

cussion upon a point of pathology of great importance, between two of the most distinguished practitioners in the State, conducted with a freedom from all that is personal and invidious, yet with sufficient spirit and occasional satire to arouse both parties to animation, and give pointed interest and zest to their productions. Surely this is far better than to listen to the erratic Dr. Oliver Wendell Holmes* let run his vein of humor, even to the extent of ridiculing, almost the idea of medicine being a science, or even an art of any value to the human race.

Although our therapeutics are not deducible from our pathology, yet they must necessarily be modified by it; we, therefore, cannot consider as of minor importance this controversy in regard to the nature of pneumonia, a disease daily met with among us, but as one worthy of our serious consideration.

We have heard men say that they were wearied out in having their notions of pathology uprooted, and some go so far as to cease to take an interest in it; but unquestionably this is wrong. Such men ought to learn to recognize the fact that the science of medicine is a progressive one, and to receive its present state as truth, although it may very shortly be upturned and pronounced erroneous. So intricate are the phenomena of life, and the working of the human economy in health and disease, that investigations of to-day seem to establish a fact which further and more scientific researches upset to-morrow. We hope we are getting wiser everyday, and we are accomplishing much if we are only advancing fast enough to learn our own ignorance. We have been so fastened down to routine in practice and to theories in belief, that it is difficult work to extricate ourselves. But no one can deny the rapid strides medicine has been making within a few years in all its departments. The stethoscope, chemistry and the microscope are all bringing to a focus their respective rays upon our pathology, and clearing up its obscurities, and thus assisting us in the direction of our shafts in therapeutics. He is behind his day who refuses to move onward and give up his prejudices, even

* An Address delivered before the Medical Society of Massachusetts, May, 1860.

when Trousseau tells him through the French Academy, that such distinguished chemists as MM. Favre and Reveil have demonstrated that in chlorosis there is no actual deficiency in the amount of iron in the blood. We were, therefore, by no means much shocked to be told by Dr. Manson, that although he did not propose "anything striking or original," yet he had made an important discovery, which was, that at the South we had all been making fatal mistakes in not recognizing a form of pneumonia which he christens "Malarial Pneumonia," differing from all others heretofore acknowledged—so materially differing from them as to be "unlike in character and in nature." Had he have stated that inflammation of the tissue of the lung in a subject under the influence of malaria was modified by that poison, it would have been a truism. Doctors Boling and LaRoche have shown us this, and the fact has been for a long time recognized by all prominent writers of monographs on pneumonia and of systematic treatises of medicine. We all daily see that disease, like the growth and the structure of vegetables, is modified by the soil from which it springs.

This statement of Dr. Manson struck us as a bold, manly conviction of an independent and fearless spirit, and we at once became deeply interested, although with the profession generally, we had flattered ourselves if we knew any disease familiarly and thoroughly well, it was this, which, since Laennec's brilliant discovery, had received so much attention. The auscultatory signs in its various stages had rendered its diagnosis ordinarily so easy, and particularly had we so long associated with pneumonia the "*râle crépétant*," that we have generally concluded if we did not hear it, either the inflammation of the lung was central, or else that previous to our examination it had rapidly advanced into the second stage. But we are gravely told by Dr. Manson, that in "malarial pneumonia" this "crepitant rhonchus is not usually present at the commencement, or, indeed, in any of the stages of the disease." He afterwards admits that, as the exception, these dry crackles, insignificant in size yet significant in value, are

sometimes heard, but "in many cases the practitioner will fail to observe any morbid sound." This, to us at least, novelty in pneumonia, he accounts for on the supposition that "this affection is doubtless an intense congestion of the parenchyma associated, perhaps, in some degree with inflammation." Yet he speaks of fatal cases where, although the dyspnoea had been extreme, the vesicular murmur had been normal, with no crepitation! In the second stage of this form of pneumonia, Dr. Manson says, "In a large majority of my cases neither bronchopony, bronchial breathing nor bronchial cough have been observed." This anomalous fact he accounts for on the supposition that in this variety of pneumonia the permeability of the lung is due to excessive engorgement, and not to lymph exudation. Thus the lung does not possess the density requisite to make it a good conductor of sound. Such being the condition, the third stage of pneumonia he but rarely met with.

In his remarks upon the differential diagnosis of this new disease, Dr. Manson dwells upon the character of the pulse, which he says is "frequent, soft, of moderate fullness, compressible, and never or rarely full, tense or hard." Next he speaks of its paroxysmal character, and the "regularly recurring cold stage and the evidences of gastric and hepatic disorders, which have gained for it the epithet of 'bilious.' The expectoration is usually less viscid, less copious, and more fluid, partaking more of the character of blood mingled with serum." The presence of delirium of an intermittent and neuralgic character, he speaks of moreover as diagnostic. Our readers will perceive, that Dr. Manson does not describe an ordinary pneumonia, such as all acknowledge occurring among malarious patients at the South, but a new disease, without the usual phenomena; in consequence of which fact, it has not been recognized, but has been treated heroically, to the great injury of the sick. It is not asthenic pneumonia, nor yet is it typhoid pneumonia—it is something *sui generis*. We do not know how more appropriately to designate it than by giving it (*à la* Bright and Allison) the name of its discoverer.

Does Dr. Manson establish his point? He certainly starts out with a very grave charge against the medical profession. With him lies the *onus probandi*, and we must confess our disappointment in reading over his papers to note that he has not given us the details of a single case, nor yet does he appear to have made a single autopsy! We assuredly have had enough of this generalizing in by-gone days; it has been the bane of our science, and if Louis had done nothing else but establish the Society for Medical Observation, insisting that we should have portraits of disease presented to us, methodically drawn out from day to day, with full specific details of all the symptoms and signs, and that we must utterly reject all statements not based upon such facts,—if he had done nothing but this for us, he would have deserved his enviable reputation and our gratitude.

Dr. Manson's expressions, "sometimes," "doubtless," "frequent," &c., do not give us ideas of his disease. Had he have given us, at least, specimen cases, and then an analysis of a large number, stating exactly the number observed in which the crepitation was absent; in how many the expectoration was sero-sanguineous—the average of fatal cases, &c., &c., we might have had, perhaps, some conception of his disease. But the strangest part is, he states that this pneumonia(?), as a general rule, is not an inflammation, and consequently we have not lymph in the air cells, when he does not mention ever having seen one of these wonderful lungs, even with his naked eye. Whereas he should have imitated Gardner of Edinburgh, and still more, DaCosta of Philadelphia, and have let his own penetrating lens have been assisted by some artificial ones, such as Nachet or Spenser could have afforded him. The day has passed when the medical world could be content with such vague, loose and indefinite descriptions of disease. It desires something more accurate, just as the community generally are not easily satisfied in their curiosity in regard to new personages and things. The Japanese, the Great Eastern, or even the Prince of Wales, arrive on our continent, and we must have, not merely ordinary newspaper accounts of them, but we

insist upon the illustrated newspapers giving us likenesses of them, drawn with vivid accuracy, so that we may at once recognize the lions when they come across our path. We are not prepared to say that Dr. Manson has not made a discovery, but we must acknowledge, that he has signally failed to portray to our eyes the disease with anything like photographic accuracy. It may be our stupidity and obtuseness, but we do not think if we should ever meet with "Manson's Pneumonia," that its features would be familiar to us.

Although Dr. Manson himself says he "cannot advance any claim to expertness in physical diagnosis," yet we feel confident that when he says there is no crepitation and no bronchial respiration to be heard, that it must be the fault of the lung and not his; but we confess we are not prepared to admit that the said case was one of pneumonia, in any sense of the word. And this, too, when we do not believe in any signs being absolutely pathognomonic. Physical signs are of inestimable value, but they are significant of physical conditions, and not of pathological lesions; which must be arrived at by due weight being assigned respectively to both the objective and the subjective symptoms, together with a careful searching into the post-mortem appearances. If Dr. Manson had spoken of his disease as a passive congestion of the lungs caused by malarial poison, in and around which there is occasionally met with, and consequent upon it, some inflammatory action, constituting a form of asthenic pneumonia, we think it would not have had the nondescript appearance which it now has. We beg the Doctor to write out in detail his cases, and give us the results of his autopsies, and then we shall be able to judge more clearly of his views.

Such were our impressions in going over the original articles of Dr. Manson. After having read, with great interest, the review of them by Dr. Howard, we feel compelled to say, that it is a masterly refutation of Dr. Manson's ideas about malarial pneumonia in all its bearings, together with a clear, logical analysis of the whole subject. He takes up statement after statement and questions them, sustaining himself by quotations from all

the principal authorities, showing a familiarity with the disease and all that has been written about it, both at home and abroad. We have in Dr. Howard's review a thorough examination of the different physical signs and general symptoms of pneumonia, with their correlative value, which makes the volume of great interest, containing, as it does, an amount of information on this disease, which we would have to seek for and cull from many authors. For his treatise, for such it is, the profession will be grateful as a valuable addition to our American literature on this prevalent disease. We had noted a number of points where we thought Dr. Manson had rendered himself vulnerable to criticism, but after reading Dr. Howard's papers, we find he has literally left nothing for us to take hold of! Indeed, it appeared to us, that although Dr. Howard is generally very fair, yet that in some places he had, with lawyer-like pertinacity, for what he considers sound pathology, ascribed to Dr. Manson intentions of conveying a meaning which certainly was not apparent, on the principle, we suppose, of Tennyson,

"That words, like nature, half reveal
And half conceal the soul within."

We do not think Dr. Howard gives Skoda due credit for his theory of consonating sounds, which, although they may not be true to the extent to which he would draw them, yet some difficulties in regard to the conduction of sounds are at least plausibly explained by them. In therapeutics Skoda is, like Dr. Holmes, a skeptic, but with less excuse; for the latter, if we are correctly informed, has not for many years been a practitioner. It certainly is illogical to conclude, as these distinguished gentlemen appear to have done, that because it is undeniable that the desired effects do not invariably result from the application of remedial agents, therefore there is no truth in therapeutics. For similar reasons, as it seems to us, the eminent Professor of Vienna is unwilling to acknowledge the real diagnostic value of the crepitant rhonchus, because it is not always perceived, and because there may be cases where it is possible to confound with it a fine sub-

crepitant râle of capillary bronchitis. To us the crepitant, as *Chomel* says, is found in almost every case of pneumonia, and is, moreover, the clearest and most reliable in the whole list of physical signs. We have, as characteristic of it, the extreme minuteness of the crepitations, their great number, their uniformity in size, their dryness, their rapid succession each upon the other, their bursting under the ear, their increase in intensity from the moment they commence on to the end of the inspiratory act, their close resemblance to Williams' comparison of the crackling of the hair between the fingers, and together with their vesicular character, the fact that they are not removed by coughing or by expectoration.

We are surprised that Dr. Howard does not controvert Dr. Manson's statement that this fine crepitation is produced, as *Lennec* believed, by the passage of air through fluid in the vesicles and in the terminal bronchial tubes, making it to differ from sub-crepitant and mucous rhanchi only in the minuteness of the bubbles. Whereas the truth is, that bubbles is not the idea conveyed to our sensorium by the sound, but rather minute cracklings. As *Walshe* so well expresses it, "rather of delicate tissue undergoing minutest ruptures with a crackling noise, in many points, simultaneously." Dr. Howard alludes to the fact, which all who have been much in the habit of applying the ear to the chest must have noticed, that we frequently meet with a fine rhonchus, closely resembling that of the first stage of pneumonia, at the end of the first few deep inspirations in healthy subjects. This cannot be produced by air passing through fluid, but probably by the dry walls of the vesicles, not dilated in ordinary breathing. Dr. Carr, the originator of auscultatory-percussion in fractures and solid organs, has furnished the most satisfactory explanation* of the physical production of the true crepitant râle. He suggests that it is owing to the abrupt separation of the walls of the cells, which had become adherent by means of the mucus or viscid exudation incident to the early stage of

* American Journal of Medical Sciences, October, 1842.

inflammation. We are confirmed in our belief on this point by very high authority, for we find that Dr. Austin Flint, in his original and invaluable treatise on the respiratory organs, adopts it. If we moisten the thumb and fore-finger with any paste, and alternately separate and draw them together, we have an exact imitation of the fine crepitation. Plastic exudation, it must be borne in mind, when first deposited as the result of inflammation, is not solid, but semi-fluid and glutinous, but becomes firm in a short time. Such is the case in the first stage of pneumonia before the vesicles have become perfectly impervious to air, and it cannot be removed by expectoration; and the sound can only be heard in inspiration, for that is the only period that the agglutinated walls of the vesicles are separated. This accounts, moreover, for the fact, rare though it is, that we may have a sound closely resembling the crepitant in œdema, and in apoplexy of the lung, for the albumen of the serum effused in the one case and of the blood in the other would agglutinate the walls of the vesicles.

We do not wish to extend this article beyond its proper limits, yet we must call attention especially of those who practice in malarious regions to the ill-effects Dr. Howard points out from the use of mercury in the treatment of Southern diseases. He says, "that it has a marked tendency to bring on inflammation of the large intestines, dysenteric straining, with mucous and bloody stools, if given in doses of fifteen or twenty grains, and especially if it be long continued, even in smaller doses." "Indeed," he adds, "it is chiefly in consequence of this effect of mercury and the marked impressibility of the bowels in the South that mercury has been for a great length of time slowly falling out of use among us." This is an item for Dr. Habersham to insert in the next edition of his work on the abuse of mercurials; for there are, we fear, those to be found who, instead of recognizing bilious, greenish-mucous stools after calomel, as caused by its irritation, consider them as indications for its continued use even to the extent of which Dr. Howard warns us.

Dr. Howard is very cautious in his recommending blood-

letting in pneumonia, as met with in the South. He endorses, in this respect at least, Dr. Manson's views: "If the pulse is hard, full or tense, as it is in the exceptional cases, or if the patient is robust, previously healthy, of ordinary vigor, and the pain or dyspnœa very intense," bleeding is justifiable, "so as to moderate, and without any expectation of cutting short the disease." Were it not for this last redeeming sentence, we should be disposed to find fault with him for speaking of venesection as "the potent remedy," and again as the "*remedium magnum*" in pneumonia. With all due respect for the opinions of these prominent practitioners, we must question the propriety of ever resorting to blood-letting in pneumonia where the subject's power of reacting has been lowered by malarious poison. We are confident that they themselves never resort to it in other cases of idiopathic poisons, such as scarlet fever, small pox, &c., no matter how bounding the pulse.

Dr. Howard says, with great truth, "like everything else, however, blood-letting has been used in every age to great excess;" he calls this "a profuse and preposterous expenditure of the vital fluid," and adds, "every one knows that within a few years past a great change has taken place in the treatment of inflammatory diseases, especially the practice of bleeding." "During a recent sojourn of some weeks at the Virginia Springs, we conversed with physicians from different portions of the country in regard to bleeding in inflammatory diseases, and especially in pneumonia, and did not meet with one who had not been obliged to discard the lancet altogether, in the vast majority of cases, or to use it very sparingly in those instances admitting of its employment at all. Even among those living in the mountains, such was the experience of all with whom we had any conversation. There never was a greater change in the therapeutics of inflammatory affections than the past few years have evinced." This is very valuable testimony from accurate observers, and we have had of late in the blood-letting controversy much to the same import. How are we to account for this great change in treatment? Dr. Howard concurs in the opinion of those who think that

disease has changed its type and is less sthenic than formerly, and consequently demands less active treatment. This may be a correct explanation, but it is a very singular fact, if true, that there should have been such a wonderful revolution among the morbid causes everywhere, in both continents; among the robust English, living on beef-steaks and brown-stout; among the nervous, active French, with their highly seasoned morceaux de viande and light wines; as well as among the fat Germans, swilling lager beer and eating their favorite sauer-kraut.

Again: on this side of the broad Atlantic this mysterious and unaccounted for depressing epidemic has spontaneously broken out simultaneously with its appearance in Europe, and we find the industrious Yankee braced by his cold atmosphere for the "irrepressible conflict," sharing alike with his more self-indulgent Southern neighbor, relaxed though he be by his warm climate, in this blessed armistice with the lancet! But it extends not only to pneumonia, but to all inflammatory diseases. Who ever bleeds now in rheumatism, in dysentery, epidemic puerperal fever, &c.? Indeed, look at the treatment of what are called idiopathic fevers. Is it not now admitted everywhere that in them the abstracting of blood from the arm is highly injurious? Have the diseases of the brain changed their nature too? Have Rowland's, Durand-Fardel's and Todd's researches on "ramolissement cerebri," "atrophic softening," &c., had no effect in changing the type? And have Lebert and Robin in France, and Paget in England not brightened up our ideas upon this subject, and enabled us, if not to discover the weak points in our own brains, at least to detect the soft places in others, by demonstrating minute arterioles made brittle from gradual fatty degeneration, causing ætheromatous deposits in their middle coats, and consequently impaired nutrition of the cerebral structure? Not many years since nearly all cerebral seizures were considered as the result of congestion, and all paralyses as caused by apoplexies produced from plethora, and venesection was the prompt remedy; but who thinks so now, or who dares so to act, without first carefully seeing if there is no hypertrophy of the heart or atrophic softening?

We would ask Dr. Howard, if his views of the antiphlogistic treatment have not undergone a very marked change since he left the Alms-House Hospital, where as a student his promise for eminence in his profession was acknowledged? Did he not there see patients bled time and again in the second stage of pneumonia, and that too when they were so weak that they had to be stimulated to enable them to sit up to have their arms bandaged? Unless we very much misunderstand him, he would now pronounce such treatment rather too heroic, for he speaks of "no expectation to cut short the disease" by blood-letting, and only of moderating the intensity, &c. Are we not unconsciously in this age of restless activity carried onward by the waves of science, although there may be "currents and counter-currents?" The man looks on from so elevated a position and with a vision so much more discerning, that he thinks the objects themselves have changed since he was a boy, when it is he that has advanced in stature and in wisdom. We mean no disrespect to those giants in medicine who have preceded us, and even if we (modern medicine) only estimate ourselves as the dwarf on their back, yet we fancy we can see farther than they did in their day and generation.

As calomel has been supplanted by quinine, because observation and experience have proved it more valuable in malarious diseases; and as formerly "dropsies" were treated as one disease, and now we encounter them as distinct diseases of the heart, liver, kidney, &c.; so is it with what are called inflammatory affections. The increase of fibrine in the blood, supposed formerly to be evidence of inflammation, is now known to occur in other conditions entirely different, such as in pregnancy; and curious enough, where it was least suspected. The late *Dr. Frick* showed it is met with in purpura hæmorrhagica. Under all these circumstances, and whenever the natural proportions between the fibrine and the red globules is altered as in anæmia and chlorosis, we have the "buffy coat," which can, therefore, be no longer considered as indicating and demanding the use of the lancet. On the contrary, analyses of the blood show, beyond a question, that venesection does not de-

crease the quantity of fibrine, but it does rapidly diminish the red globules, which are not increased in inflammation, and which, in their normal proportions, are essential to the healthy vitalizing state of this river of life; and, moreover, this same "buffy coat" is increased in amount and in firmness as we lessen the number of red globules—that is, in proportion with the quantity of blood abstracted.

Again: Blood-letting acts mechanically in inflammation, by reducing the whole amount of blood in the body, and consequently in the inflamed part; but nature, in a very short time, supplies this deficiency in quantity by absorption into the circulation of the other fluids of the body with their inorganic constituents. But the quality of the blood (and herein lies the injurious effects of venesection) is not so easily restored to its healthy standard, for the red globules have to be manufactured, and this is slow work in the depressed state of the organic functions, as is every day evidenced by the prolonged weakness and by the pallid cheeks of those who have been bled. When, therefore, venesection is not imperatively demanded, and we can control the heart's action by such remedies as tartar emetic, digitalis, veratrum viride, &c., they are substituted for it.

Dr. Stillé, in his admirable treatise on therapeutics, which we so cordially welcomed last month, has hit upon one great cause of this change of treatment. In speaking of the antiphlogistic treatment of pneumonia, "we are told," he says, "that the first principle in curing an inflamed organ is to give it rest, but as there is no rest for the lung, reason requires us to diminish its labor by abstracting blood, and accordingly depletion is held to be the capital remedy for pneumonia, and experience has for centuries appeared to agree with reason in sanctioning this practice. But when once the natural cure of pneumonia had been studied, it became apparent that the loss of blood is very seldom essential to the patient's recovery, and that it is more generally mischievous than useful in any but the forming stage of the attack." This has been told us not only by such men as Bennett and Markham, but assuredly MM.

Louis and Trousseau, Sir John Forbes and Dr. Todd were not enthusiastic hasty theorists, young in age and inexperienced in practice, but all of them men of much vigor of thought, with matured judgments controlled by great regard for truth, to ascertain which they spared no labor as close observers of nature and of disease.

Most unfortunately for the defenders of the change-of-type theory, there are practitioners to be met with everywhere who have stood still for years, coiling themselves up in a very uncertain looking mantle, which, in their humility, they call conservatism, and with which they veil from their eyes what is going on in the medical world. The lancets of these men do not rust in their cases; they bleed whenever they meet with a hard pulse, even though it be caused by aortic stricture, or indeed wherever there is "arterial excitement." This too with their every other diagnosis 'the liver' and their every prescription a mercurial alterative. We have heard of two practitioners, with long lists of confiding patients, ridiculing, even since Dr. Howard wrote his papers, the idea of curing pneumonias without venesection; and very recently we have known of a poor anæmic woman bled until she fainted, because she had some slightly discolored expectoration, which was supposed to indicate tubercular phthisis, and the practitioner had been taught not many years since that this was an inflammatory disease which must be cut short—its phlogosis must be extinguished! Judging from the tone of Dr. Holmes' address, and from his low estimate of the standing of the profession of medicine, we fear that these bright exceptions are by no means rare in his intellectual modern Athens. These men are not stupid, they are shrewd, and why do they not see this change from sthenic to asthenic? Do we not see, in greater or less number, everywhere, these rocks, whose solid firmness excites our admiration at the same time that their imperturbability tries our patience. We would not have them carried away by every passing wave, but it would not lessen our respect for their fixed principles to see the marks of the heavings of the surging tide left upon them; for assuredly stolid immobility is not conservatism. They doubtless have some influ-

ence of momentarily stopping the onward progress, but it passes them, leaving them where they will ever stand, until they crumble into dust. Examine them closely, and you will not always find them covered with the gray lichens of age and

“With beards as youth gone out
Have left in ashes.”

Doctors Manson and Howard speak highly of quinine in the treatment of pneumonia; the former gentleman in estatic enthusiasm, not only to counteract the malarious element, but “even in certain cases of pneumonia of a more or less asthenic type, which present no tokens whatever of the element of periodic fever.” But as all pneumonias now-a-days are of this low type, it follows, not only that we must not bleed, but must actually give quinine, which is a stimulant and tonic, to support the powers of life so as to enable them to bear up against the depressing influence of the disease. Consistent, concomitant practice with this is the administration of animal food in its most digestible form, such as beef-tea; but the digestion is weak, and even fluid animal broth has to undergo digestion. We want an article of food not requiring digestion and very assimilable, and we have it in the shape of alcohol. Todd, Lewes, Marcet and our distinguished friend, Dr. Wm. A. Hammond, tell us it is food when given at regular intervals in moderate doses, and we daily meet with poor drunkards who literally live on it. Its power of upholding and sustaining the vital forces, especially that connected with the nervous system, is wonderful; and, moreover, its hydro-carbonaceous nature gives it great heat-making properties, and thus the tissues, and with them the patient’s strength, are sustained. Its influence in this way in phthisis is generally acknowledged, and no less high authority than the late distinguished Dr. Robt. Bentley Todd tried it in pneumonia in repeated cases, and he pronounces it a valuable accessory in its treatment. He did not teach that it was an essential element in the treatment, as some have misrepresented him as saying. Dr. Austin Flint also reports a num-

ber of cases in the N. Orleans Journal successfully treated by this agent.

Does not all this prove that we do not yet understand clearly the exact nature of inflammation, much less its treatment? We noticed a curious fact related by Bernard in one of his recent lectures. He accidentally left three dogs fasting together for days; their nutrition failed and each of them had an acute inflammation, one a pneumonia, another a peritonitis, and a third an enteritis. We commend these facts to Doctors Manson and Howard, and we would like to have them try, in their pneumonias, which demand (they say) stimulating and supporting treatment, the effect of alcoholic food and report their results in our Journal.

ART. II.—*Endemic Jaundice Occurring in Richmond, Va.*

By THOMAS POLLARD, M. D., Richmond.

As far as the writer is informed, endemic or epidemic jaundice is extremely rare. After a search through many numbers of the Lancet, Medico-Chirurgical Review, and Amer. Jour. Med. Sciences, and the Nos. of the Virginia Medical and Surgical Journal, no account is found of jaundice in this form. Wood in his Practice, in a note (vol. ii. p. 517), says that Dr. Corson of Montgomery co., Pennsylvania, informed him, "that an epidemic of jaundice has recently prevailed in his neighborhood. Within a limited district, not more than a mile long by a quarter of a mile broad, occupying a hilly ridge, he had attended in a short time 27 cases. The disease was mild, and all his patients recovered." It will be observed that the term epidemic is here used synonymously with endemic, as it is also by other authors. Watson does not mention the disease in epidemic form. Dr. Dickson (vol. ii. p. 176) speaks of malaria as producing icterus, and says that Cleghorn mentions "a common distemper in July and August of the year 1746, a

slight jaundice without fever, which soon yielded to purgatives and saponaceous medicines." This was in Minorca. Dr. Dickson goes on to say, "A similar prevalence of jaundice was observed in this city in the autumnal and winter months of 1824. During the season we had been visited with bilious and yellow fevers, and convalescents from both these were almost universally affected with jaundice as the cold weather set in. Nor were they the exclusive subjects of such attacks, for many were similarly assailed who had previously enjoyed good health. The cases were usually mild and manageable, but their great number seemed to me unaccountable, except by reference to such a condition of our atmosphere as Sydenham would have called 'epidemic.'" Budd, in his work on the "Liver," says, "Now and then it occurs with peculiar characters in several members of a family, or in several persons living together, in quick succession—where it can only be attributed to some peculiar poison." This does not, however, describe well-marked epidemic or endemic jaundice.

Valleix (vol. iii. p. 255) says: "Icterus may be developed in an 'epidemic' form. It is certain that at particular epochs we see appear a number of cases, often considerable, of icterus, perfectly simple in character, the progress of which presents nothing striking, and the termination of which is fortunate. We saw not long since, in the hospitals of Paris, a limited epidemic of this kind. Sometimes in these epidemics the icterus assumes a grave form."

These extracts are sufficient to show that jaundice does prevail occasionally in epidemic form, and its rarity is proved by the failure of many authors to speak of it; and by the fact that very few medical men have encountered it in their practice.

The cases about to be described occurred in this city in the month of June last, at the stable of Mr. Bossieux, situated between 20th and 21st st., fronting on the Dock. The first cases which were treated there occurred in May last. The writer saw some of them for a medical friend who was absent from the city. Most of them were attended with fever, furred tongue,

and generally constipated bowels. Occurring in the first warm weather in May, in connection with the locality of the stable near the water, the somewhat apparent remission in the fever, caused them to be mistaken for remittent fever—though some of the marks of remittent fever were absent. They were treated with calomel and quinine. In a few days, perhaps in a week, in most of the cases (five in number), yellowness of the conjunctiva began to be observed, and the true nature of the disease was then first discovered. In connection with the symptoms just mentioned, the urine was found colored with bile, the spirits were much depressed, debility was marked, appetite gone, and the secretion of the liver deficient, and the passages dirt colored—such symptoms, indeed, as are usually found in this affection. Convalescence was slow, and the patients (likely negro men), were a long time on the sick list, considering the mildness of the symptoms, and the fact that they were usually able to sit up or walk about a considerable part of the day—though, as before remarked, they complained much of debility. When the yellowness of the eyes, and torpor of the liver became manifest, calomel was more freely used, and with good effect.

As the disease progressed, as is usual with epidemics, it assumed a more decided and worse form. One after another was taken down, and out of nineteen likely negro men seventeen of them had jaundice. A young white man employed about the stable also became a subject of the same affection. Five of the cases became patients of the writer, and he had an opportunity of watching their inception and progress. Four of these cases were sent to Bellevue Hospital, and with one other not sent to the hospital, and one more sent to the hospital by another medical man, will be briefly detailed.

CASE I.—Wm. Leigh, a slave, was seen first on June 9th. Found him with fever, tongue much furred, appetite gone, headache, with complaint of debility, and with decided depression of spirits. Bowels reported to be confined. Ordered him 20 grs. calomel and 20 grs. quinine, in four doses, one dose to be given every three hours.

June 10: About the same, with exception of diminution of fever. Bowels moved several times, but the liver imperfectly acting. Urine containing some bile.

June 11: About the same—conjunctiva of the eyes began to assume *yellow* color. Sent to Bellevue Hospital, where he was ordered quinine 4 grs., camphor 4 grs.—every three hours until 20 grs. of each are taken.

The calomel was continued in moderate doses until the 16th, when the liver was aroused into action. Nourishing diet was then given, as there began to be considerable depression. Toddy was also ordered at dinner, and infus. cinchona and elixir vitriol as a tonic. To this treatment was added, in a few days, nitric acid, 4 drops an hour after eating, the bark, &c. being taken an hour before meals.

The patient gradually improved—the liver slowly assuming its proper action, the bile matter leaving the urine, and the eyes very slowly giving up the yellow tint.

July 3: Discharged, not well, but convalescent. It was three or four weeks before he was able to assume his work, that of driver.

CASE II.—Was called to see Peter Price, occupation, driver, on June 10. Found him with very feeble pulse, cool skin and serous diarrhœa, which he said commenced the day before. Tongue furred, much depression of spirits, the patient speaking in a whisper, more from a fancy he could not speak louder than from inability to do so. R.—4 grs. calomel, 1 gr. of opium, and 3 grs. of camphor, every four hours, with free use of stimulants and warmth to extremities.

July 11: No better—continued treatment with addition of 4 grs. quinine to each dose of the other medicine. Milk toddy every three hours. Pulse very feeble—skin very cool.

June 12: Same condition—serous passages continue, though checked in frequency. R.—Submur. hyd. ʒj, in one dose. Pulse still feeble. Leave off pills, calomel and opium; quinine and camphor.

June 13: Calomel has acted upon the liver, and the patient has had a large, dark, consistent passage, the first of the kind

since his sickness. Pulse immediately rose; surface became warm, and the general condition is much improved. Stimulants only occasionally.

June 16: The patient's condition not much changed until last night, when he exhibited signs of delirium. There had been, for a day or two, drowsiness—attributable to imperfect action of the kidneys. From the 13th, calomel or blue mass had been daily given, with the view of keeping up action upon the liver, and occasional doses of laudanum, or opium, to prevent over action on the bowels. The passages shewed action of the liver, but not perfect, healthy action. Eyes did not begin to get *yellow* until the 16th.

On the night of the 15th, the patient's brain was so much disturbed as to induce him to escape from his window, and he was found at a late hour wandering about in the yard. Under these circumstances, it was deemed best to send him to "Belle-vue Hospital," which was done on the 16th. The following was ordered for him on his arrival there: Ol. terebinth. gtts. xv. every four hours; tinct. opii, gtts. xv., p. r. n., to restrain bowels. The turpentine was directed with a view to its action on the kidneys, to the inaction of which was probably attributable, at least in part, the head disturbance. Milk toddy every four hours. Diet—soup and milk. The conjunctivas very yellow.

June 17: Was so delirious last night as to require the straight-jacket, as he could not be otherwise retained in bed. R. Toddy every six hours; infus. cinchon. oz. ij., with elix. vitriol 15 gtts. every eight hours. Continue turpentine.

June 18: Rather better. Continue tonics, and give submu. hyd. 1 gr.; opium gr. $\frac{1}{4}$ every eight hours, as the liver seemed not to act well. Less delirium.

June 21: Patient's condition has improved. Liver acting better. Leave off calomel and the medicines, and give nitromuriatic acid 4 gtts. three times a day, and tinct. opii, if necessary, to restrain the bowels. Delirium has been relieved. Continue toddy.

Under this treatment he continued to improve. The diet

was gradually strengthened, and the patient encouraged to walk about in the yard; and, on July 3d, he was discharged. He was sent to the country, where his improvement was not rapid, and debility a long time complained of. Eyes were still somewhat yellow.

CASE III.—Was called to see Phil, a large, heretofore healthy man, on June 13. Occupation driver. Found him with fever, furred tongue, head and back ache, bowels constipated, and pulse rather active. Ordered—Quinine, 1 scr. in 6 pills, 2 of which to be given every three hours, and 2 grs. of calomel every eight hours.

June 14: Fever continues. Bowels not moved. R. Castor oil oz. ss., and repeat, if necessary.

June 15: Rather better. Bowels moved, dirt colored, with bad odor. Less fever. Eyes *yellow* to-day.

June 16: Was removed to “Bellevue Hospital.”

June 17: R. Infus. cinchon. oz. ij.; elixir vitriol xv. gtts. every eight hours.

The treatment adopted from this time was tonic, with occasional dose of calomel, to keep up action on the liver, which, in this case as in all the others, was torpid. Fever continued for some time, and quinine was again tried with the hope of breaking it up, but without success. The nitro-muriatic acid was also given in this case.

July 3: Discharged, nearly well. The eyes still somewhat yellow, and debility complained of. The improvement in this case was slow, as in the others. Depression of spirits very marked in the commencement, so much so, that the patient was sometimes found in tears, though the attack was much less severe than some of the others. A symptom complained of by Phil, not mentioned, and which was observed in most of the other instances, was soreness about the ankles and feet. As Phil convalesced, he complained of inability to see well—not yellowness of vision, as observed in some cases of jaundice. The vision, for a few days, was very imperfect, but gradually improved.

CASE IV.—Peter Lorrell was first seen on June 13. Heretofore very healthy. Occupation driver. Found him with fever, head and back-ache, furred tongue, and depression of spirits. Under the idea that malaria had something to do with the attack, he was ordered quinine scr. j., in three doses, and 2 grs. calomel three times a day.

June 14: Symptoms somewhat better. Bowels moved, and passages dirty, clay-colored. Continue calomel.

June 15: Removed to "Bellevue Hospital." Eyes began to assume *yellow* appearance. Urine colored, with bile. Complaints of soreness about feet and ankles, and pain in the back, for which dry cups were ordered. The treatment in this case consisted of tonics, infus. cinchon. co., and elix. vitriol, and afterwards nitro-muriatic acid, and occasional administrations of calomel. The bowels were irritable, for which tinct. opii and opium were given. Appetite was generally bad. He gradually improved, though complained much of debility after his fever wore off. The liver slowly resumed its function, the urine ceased to be colored with bile, the appetite improved, and he was discharged July 3.

CASE V.—Was called to see Dick, occupation driver, a remarkably large, well-developed man, on June 11. Found him with fever, active pulse, furred tongue, pain in the head and back, depression of spirits, and want of appetite; bowels rather torpid. R. Sulph. quin. scr. j, submu. hyd. grs. xv., in six pills, two of which to be given every three hours.

June 12: Fever less, bowels moved, and head-ache not so much. Passages dirty colored and offensive. R. Submu. hyd. grs. 3, night and morning.

June 13: Head-ache still continues; tongue very much furred. Bowels moved, but not improved in appearance. Depression of spirit so marked, as to find him sometimes in tears. Complaints of pain and soreness about the legs, and more particularly about the ankles. Fever less. R. Submu. hyd. grs. ij., twice a day.

June 14: Somewhat better, though still fever, and furred

tongue, and head-ache. Passages still dirty in color, and offensive. Continue 1 gr. calomel night and morning. The treatment consisted from this time in tonics, with about 1 gr. of calomel a day, (as the secretion from the liver began to improve,) until the passages became more natural. The eyes, in this case, began to be *yellow* about the 15th. Previous to this, the urine began to be colored with bile. It was found necessary to give toddy when the fever declined, as the debility was marked in this, as in all the other instances. Improvement was very gradual, and, on the 25th, the patient was dismissed, in a fair way to recovery—the liver acting, the yellowness of the conjunctiva disappearing, the urine becoming natural, the appetite and strength returning.

On the 2d July, however, was called again to see Dick, who was suffering from diarrhoea from some imprudence in diet. Many of his bad symptoms had returned, and it was necessary to go somewhat over the same treatment. This second attack yielded to remedies, and about the middle of the month he was discharged, slowly convalescing. In this case the patient complained also of imperfect vision, not yellow vision.

CASE VI.—Peter Haskins—occupation driver—was admitted into “Bellevue Hospital” June 15. Has been sick for about a week, with fever, depression of pulse, and considerable vomiting. When admitted, he complained of pain in the head and back, with bowels rather constipated. R. Dry cups to back, to be followed with mustard plaster, and 15 drops oil turpentine every eight hours, with castor oil \mathfrak{z} ss. Diet light, but nutritious.

June 16: Bowels not moved; condition not improved, and pulse feeble. R. Submur. hyd. grs. v., every eight hours until it acts. Toddy every four hours, until pulse rises.

June 18: Bowels mixed, but liver not secreting well. Pulse improved in strength. R. Submur. hyd. gr. 1; opium gr. $\frac{1}{3}$, every eight hours, and 4 drops nitro-muriatic acid, between the times of the other medicine. Toddy once a day. Vomiting quite frequently, for which was ordered hydro-cyanic acid gtts. ij., every four hours. Eyes jaundiced to-day.

June 20: Symptoms worse. Prostration very great. Bowels too loose, with still some vomiting. R.—Continue medicines, and give milk toddy every two hours. Conjunctivas of the eyes very yellow. Disposition to drowsiness. Has been some epistaxis during the morning. Surface cool.

June 21: Symptoms more unpromising. Some hæmoptysis, and some blood in the passages. Muttering delirium, with somnolency. Continue calomel; leave off the nitro-muriatic acid; continue turpentine, and give 2 grs. of sugar-lead every two hours, Toddy as before, and beef-tea frequently. Hydrocyanic acid when there is any vomiting. Pulse very feeble and surface cool.

June 22: Hæmoptysis continues, though there is nothing morbid in the sounds of the lungs except some mucus rales. Still blood in the passages, which are too frequent. Liver acting very imperfectly; so of the kidneys, the secretion from which is small in quantity, colored with bile. Continue medicines, and add infusion of juniper to the turpentine, which is given every four hours. Diminish calomel to $\frac{1}{2}$ gr. three times a day. Apply blister to the liver and abdomen, 6x8, with the view of rousing the organ to action.

June 23: Symptoms somewhat improved. Hæmoptysis nearly subsided. Bowels in better condition and not much blood. Liver still secreting imperfectly. Increase the calomel to 1 gr. Delirium nearly gone. Still somnolency. Eyes very yellow.

June 25: Some improvement. No blood from bowels. Pulse stronger. Toddy now every four hours. Give acet. lead 2 grs., submur. hyd. $\frac{1}{2}$ gr., oil turpentine 20 gtts., three times a day, to be washed down, the infus. juniper with $1\frac{1}{2}$ oz. gin mixed with it. Kidneys still acting imperfectly. Diet to be increased; appetite begins to return. Somnolency much less, and he begins to take more notice.

June 30: Slowly rallying. Liver acting better. Leave off calomel. Resume nitro-muriatic acid, and continue turpentine, juniper and gin.

Convalescence had now fairly set in, and the treatment was hereafter principally tonic, with an occasional mercurial and

tinct. opii, p. r. n., to restrain the bowels, which were yet irritable. It was also necessary to use subnit. bismuth, which was ordered July 16, for this irritability of bowels.

The medicines, turpentine, juniper and gin with toddy, (and the bismuth and tinct. opii occasionally) were kept up until 29th of July.

July 30: Discharged, convalescing, but still weak and looking badly. We learned it was sometime before he was able to resume work.

Whether the hemorrhage from the bowels, nose and lungs, in this case, was the result of obstructed circulation through the liver and plethora of the venous system, or the effect of the condition of the blood, is a matter of doubt. It was more probably, however, the result of blood poisoning, particularly as the first hemorrhage was from the nose.

This man was exceedingly ill, and for several days seemed moribund—his pulse flickering—his surface cold, and he entirely insensible to external impressions.

REMARKS.—Cases such as those described cannot depend on any of the ordinary causes of jaundice. It is not reasonable to suppose there was any obstruction in the ducts of the liver or gall bladder, either mechanical or inflammatory, and it is more probable that the cause, whatever it was, acted upon the liver and prevented its elimination of bile. If this supposition is true, it supports the theory that the yellow coloring matter of the bile exists already formed in the blood. That it is true, is rendered very probable by the fact that seventeen out of nineteen persons living together, had jaundice, which must have arisen from some common cause acting on the secretory function of the liver, or poisoning the blood.

What the predisposing and exciting causes were in these instances it is almost impossible to discover. The stable in question was not more than usually foul—manure had accumulated about it, as we often see about the stables and farm yards in the country. There were no pools or stagnant water about it. Some of the subjects of the disease slept at and

about the stable, some at their owners, or at their "wives' houses." Some eat at the stable, and some elsewhere, not using any common eating house. Lying near the river, malaria in conjunction with some very hot weather occurring in June last, about the time of the first cases of sickness, and afterwards, may have had something to do with the development of this sickness. But then other localities near by, and other stables near the dock, entirely escaped any similar affection. Thus is added another evidence of the difficulty of understanding and unravelling the causation of disease.

It has been said that jaundice is only a symptom of disease, and must be treated accordingly. Here it seemed the disease itself, unless we consider this to have been a fever, the result of blood poisoning, and the jaundice one of the symptoms and evidences of blood poisoning. It will be observed that fever was a prominent condition in all the cases, and that it existed some time before there was any jaundice.

The writer has recently learned that there has been during the summer, in some of the adjoining counties, more than an usual amount of jaundice.

ART. III. *Obstetrical Memoranda.* Frequency of Accidents, or Irregularities during First Labors. By RICHARD MC-SHERRY, M. D.

IN looking back over my notes, I could not but observe with some surprise, to how great an extent primiparæ are more liable to accidents than multiparæ. The first labor is, of course, the test of the woman's capacity to bear children; and if she passes through the first successfully, the presumption is, that she will do well subsequently. But, with a full appreciation of the facts, I was not prepared to find so great a difference, practically, between the two classes of cases. In my own practice, I have had notable irregularities or disturbances

to contend with in more than one-third of the first cases; from the last fifty of which, I present a synopsis of not less than seventeen, in which there was more or less deviation from normal labor, while in other cases, my notes do not show any notable disturbance in more than ten *per cent.*, or at most, in more than eleven cases in the hundred.

In the following memoranda, I only propose to offer the most important points connected with each case, without any attempt at close or minute detail:

CASE I.—Mrs. D.; Jan. 10: Child (female) dead-born at the end of a tedious labor from close wrapping of the cord about the neck. Artificial respiration attempted without success.

CASE II.—Mrs. H.; Feb. 23: Child (male) successfully delivered by forceps. The mother had convulsions, and was actually in convulsion when I took away the child. The health of the mother was bad; she was dropsical, and had hypertrophy of the heart. The external parts of generation were enormously distended with dropsical fluid. The child when removed was very feeble, but was revived by suitable stimulants.

The mother continued subsequently in bad health until she died in December; an attack of pleurisy having supervened which hurried the fatal issue. Meanwhile, I had been obliged to tap her frequently, seventeen times in all, from April to December, drawing off each time from three to four gallons of fluid. I did not suppose, when attending this woman, that it would ever be asserted as a rule, *that the heart is normally in a state of hypertrophy during gestation.* Yet such is said to be the fact, based upon observations *made by M. Larcher and M. Ducrest, at the Paris Maternité.**

CASE III.—Mrs. M.; May 11: Child (male) born at the end of twenty-four hours of sufficiently active pains. The mother was nearly forty years of age; and the soft parts were, consequently, unyielding. The waters broke during the first hour

* The dropsy was supposed to depend on the cardiac lesion, so that the urine was not examined for albumen.

of labor. The foetal head before birth had a peculiar feeling, the bones appearing to spring sharply after being indented by pressure of the fingers. The head was large, (a family characteristic,) but I did not then suspect disease. In June, July, and August, however, I treated the child, without success, for hydrocephalus, when the family removed. The child died subsequently.

CASE IV.—Mrs. H.; Sept. 18: Child (male) born at the end of a lingering labor, with cord looped around the neck. When the child was born, I observed an immense bluish mass covering the abdomen, and upon examination, I found the bowels protruded and deeply congested. They had escaped by a rent at the side of the umbilical cord, through the linea alba. I restored them by gentle and patient manipulation, and applied a compress and binder, but the child died at the end of a half hour, without ever having cried.

The mother had been long suffering in her digestive organs, and during the entire labor was vomiting large quantities of green and yellow bile and mucus.

CASE V.—Mrs. W.; Oct. 16: Child (female) delivered, at the end of twenty-four hours' labor, by forceps. The child was quite dead; the difficulty and delay were due to its immense size. The mother soon recovered.

CASE VI.—Mrs. M.; Dec. 15: Child (male) safely delivered by forceps at the end of forty-six hours of labor. I had used ergot freely, but unsuccessfully, before resorting to the forceps. When the child was born there was no pulsation in the cord, but the usual measures of restoration soon had the desired effect. Any longer delay in using the instrument would probably have proved fatal to the child.

CASE VII.—Mrs. S.; Nov. 5: Child (male) dead-born, at 8 o'clock P. M., at the end of a labor which commenced with hæmorrhage on the morning of the 2d. Spontaneous rupture of bag of waters occurred on the 3d, while the neck of the womb was not yet effaced. The pains continued with more or less regularity until the evening of the 4th, when they became severe and regular. On the morning of the 5th, the head

appearing to be firmly fixed in the pelvic canal, I applied Hodges' long forceps, (the soft parts being all duly prepared,) but I found that nothing short of great violence would bring the head. I removed the instrument, and during the day, the pains having nearly failed, I resorted to the use of ergot. At length, the head escaped, at the time mentioned, of large size, and much elongated from the moulding process it had undergone in the pelvic canal.

This moulding is often accomplished successfully by the plastic hand of nature, so that a large head may pass *per vias naturales*, which could not be removed by instruments without inflicting injury upon the soft parts of the mother. In this case the mother suffered for some weeks with soreness of the soft parts, and with transient fever and inability to pass urine. Catheterism and fomentations had to be used to give her relief. I dreaded vesico-vaginal fistula, but fortunately, it did not follow.

CASE VIII.—Miss —; Nov. 4: Child dead-born before I reached the house, as I was at the time attending upon the preceding case, No. 7. It was a breech presentation, and the child undoubtedly died from delay of the head, and pressure on the cord.

CASE IX.—Mrs. G.; Aug. 8: Child (male) dead-born at the end of forty-eight hours of labor. The delay was owing to rigidity of the cervix and os uteri. Mrs. G. is a middle-aged woman, of delicate constitution. Under prolonged nausea excited by the use of tart. antimon., and manipulation, the os slowly yielded, but the pains failed. I resorted to ergot, and when the head came, it was greatly elongated.

CASE X.—Mrs. T.; Oct. 7: Child (female) dead-born at the end of forty-six hours of labor. She was under care of a mid-wife, and sent for me on account of a failure of pains. Finding the head low down, and the soft parts prepared, I gave her ergot which restored the pains and brought the child. When it came, the cord was lying over the shoulders, and without pulsation. The cord was deficient in firmness, breaking upon very slight traction. The child gave no evidence of life,

by motion, or by sounds of foetal heart, when I administered the ergot.

CASE XI.—Mrs. M.; Jan. 31: Child (female) safely delivered but the mother had a series of clonic spasms until she died on the 4th of February. The mother had been subject to a disorder previous to marriage in which hysteria and epilepsy appeared to combine, or predominate alternately. In the early part of her pregnancy, I advised her friends to take her to an asylum, to be treated for *hysterical mania*, so greatly was her system excited. The advance of pregnancy, however, was attended with a modification of symptoms, which finally resulted in the issue stated above.

CASE XII.—Eliza —, slave; March 6: Child (female) born at the expiration of thirty-eight hours of labor. The mother is a middle-aged woman, and the labor was slow, without special difficulty. The placenta adhered, and the cord would not admit of any traction. The placenta being very friable and easy of laceration, was with difficulty removed.

CASE XIII.—Mrs. B.; Oct. 9: Child (female) born at the end of a very protracted labor, (sixty-eight hours,) owing, apparently, to the unyielding nature of the soft parts. The mother suffered much in these, subsequently, from the prolonged pressure.

CASE XIV.—Mrs. M.; Feb. 19: Child (male) dead-born; breech presentation. The labor lasted twelve hours. It was a sacro-posterior position; in escaping from the vulva, the genital organs of the child were directly beneath the symphysis pubis. When the shoulders came in reach I drew down the arms, leaving the head in the pelvis, where a complete rotation was effected, which brought the occiput to the pubis, and the face in the hollow of the sacrum. At this stage, unfortunately, there was a temporary suspension of pains, and although I introduced two fingers into the mouth of the child, and forced back the perineum as much as practicable, the child died while I was attempting to remove it, from pressure on the cord. I determined, from this case, never to attend another breech presentation without having my forceps at hand, espec-

ally with a primipara, where the soft parts are usually so much more resisting than in subsequent labors.

I may here remark that in the two cases of breech presentation mentioned in this paper, there were, in both, moral causes giving distress to the mothers. In the first, the loss of caste, and the bitter reproaches of the family and friends of the erring girl, were deeply felt; and in the second, the mother was mourning the absence of her husband, who was on a distant sea-voyage.

I do not believe that such things influence the position of a *fœtus in utero*, but others think differently, and find in such facts confirmation of their theoretic views.

CASE XV.—Mrs. D.; May 20: Child (male) dead-born. By the report of the mother and friends, she had been in labor from Tuesday (15th) to Sunday, (20th,) when she was delivered at 8 P. M. I was with her about two hours; the child had given no signs of life for some hours previously. When brought away, the head was large and much elongated, the cord was about the neck, and so yielding as to break when I attempted to slip it over the head. There was so much tumefaction and pain of the soft parts that catheterism was required for several days.

CASE XVI.—Mrs. H.; July 10: Child (male) was safely delivered, and without difficulty, it being somewhat premature and of small size. It died on the 27th, of syphilitic marasmus. There is nothing remarkable in this case in its obstetrical relations, but the mother at the time was suffering severely from secondary syphilis, contracted, as I believe, from the *fœtus* in the womb, which derived it, congenitally, from the father.

CASE VII.—Mrs. H.; Aug. 9: Child (male) delivered by forceps at the expiration of twenty-six hours from the beginning of the labor. Mrs. H. had been married twelve years, and is apparently over forty years of age. The head rested very high, could barely be touched through the superior strait, until the waters broke spontaneously, when it descended gradually to the inferior strait, where it remained, with little or

no further progress made, for about four hours. I delivered with forceps without difficulty, but there was no pulsation in the cord, and although the child gasped feebly several times, I did not succeed in resuscitating it. The head was as usual, where there is much delay in passing through the pelvis, moulded to a great length.

Mrs. H. called my attention to a tumor, apparently ovarian, in the right iliac region, of some months' duration; the womb had a right lateral inclination.

The labor was followed by retention of urine, with great soreness and distress, which was relieved by the use of the catheter.

It may be proper to remark here, that the fifty cases which have supplied these notes, are from all classes in society; that in round numbers, it may be said that one-half of the whole, or twenty-five persons belong to the upper classes, and that twenty-five belong to the humbler classes. By the upper I simply mean those who are in easy condition in life, and who employ servants to do their drudgery; by the humbler, those who do that hard work themselves which people in better circumstances turn over to servants. It is, I think, somewhat remarkable, that at least fifteen of the seventeen cases herein recorded, are from the working classes, among whom all household drudgery, at least, was continued during the term of gestation.

ART. IV.—*Report from the Infirmary of the Medical College of Virginia.* (Service of Dr. McCaw.) By THOMAS D. HUNTER, M. D., Resident Physician.

CASE I.—OCCLUSION OF THE "OS UTERI."

Negro woman; named Agnes; aged 47; abode, Chesterfield co.; previous health and habits good; admitted June 16th, 1860.

History.—This patient is a large healthy looking woman, weighing probably 175 pounds. She is the mother of sixteen children, with each of whom, she tells us, she had “an easy time,” having no difficulty either during gestation or labor. Recently, within the past four or five years, she has been subject to miscarriages, having as many as two or three every year. Last year she carried a child to full term, and at its birth had great difficulty, sufficient to demand instrumental aid. Since then she has not menstruated, but has at the return of each monthly period, suffered extremely for four or five days, without the appearance of the menstrual secretion.

Report.—June 16: At this time she is apparently well. Pulse natural; tongue clear; appetite good; bowels regular. Says her regular menstrual period will occur in a few days. *Ordered*, R.—Zinci sulph. gr. vi.; Aq. font. ℥j. *M. Sig.*—Inject the vagina bis die.

June 18: Has suffered all day with most violent pain in the lumbar region and lower part of the abdomen. The left ovary can be felt very distinctly through the abdominal walls. It is hard and much enlarged. Pulse quick and strong and respiration hurried. *Ordered*, Give at once magnes. sulph. ℥j.; cups over the lower part of the back; apply hot fomentations to the abdomen, and a blister over the enlarged ovary. Give tinct. opii, gtt. l., and repeat if necessary.

June 19: Much worse to-day. Slept none last night, notwithstanding she took several large doses of laudanum. Seems to be in extreme pain, countenance distressed, pulse slow and weak, skin moist and extremities cold. She describes the pain as being even worse than “labor pains,” though much of the same character. Says “she would rather ‘bring forth’ two children than suffer what she does now.” The abdomen is swollen and nodulated, and quite as large as it would be were she pregnant six months. There is also excessive nausea with much vomiting. *Ordered*, A mustard poultice to be applied to the abdomen, and hot bricks to the feet; tinct. opii gtt. l., and repeat in two hours; whiskey ℥ss. in a little water every hour until the pulse improves. R.—Hydrarg. sub. mur. gr. x; pulv. jalap. gr. x. *M. ft. pill* given at once.

June 20: Is a little easier to-day. Slept some last night. Abdomen still swollen and very sore and painful on pressure. Tongue furred; bowels not yet moved; pulse stronger and quicker. *Ordered*, ol. ricini $\bar{3}j$.

June 22: Much better. Abdomen still sore and tender when pressed upon. Bowels have been moved twice. Has lost strength and is quite feeble. An examination being made to-day, the "os uteri" is discovered to be entirely closed; the finest wire probe cannot be introduced. An operation for her relief cannot be performed now, on account of the extreme sensitiveness of the parts. It is, therefore, postponed until she is further recovered.

June 25: Has so far recovered as to be able to undergo an operation. Upon consultation, Drs. J. B. M'Caw and A. E. Peticolas determined to perforate the neck of the uterus along its original canal with a trochar. The patient being placed upon a table, with her legs flexed upon her body, a speculum was introduced. The uterus was then depressed and as far as possible steadied by means of firm pressure on the abdomen, immediately above the pubis. A long curved trochar was introduced by Dr. M'Caw through the speculum and made to enter the uterus about the centre of its "os;" it was pushed in to the depth of an inch and a-half, without entering the cavity of the womb. On account of the great difficulty of fixing and retaining the uterus in its natural position, it was thought unsafe to urge the use of the trochar further, as there was danger of the instrument being pushed through into the abdominal cavity, and liable to be followed by all the evil consequences common to such accidents; and moreover, as that period in life had arrived when the menstrual function ceased, Nature herself might relieve her, without the pain and risk of an operation.

June 30: Has entirely recovered from her last attack, and from the effects of an attempt to operate. She is now apparently quite well again.

July 9: The patient is complaining to-day of the symptoms of the approach of another monthly period. She says her

habit has always been to menstruate every twenty-one days. Has acute pain through the loins and bottom of the abdomen. The right ovary, in this instance, is much enlarged. Pulse quick; tongue furred; nausea and cold extremities. *Ordered*, Cup and draw blood freely from the lumbar and iliac regions. R.—Ol. ricini \mathfrak{z} j.; ol. terebinth. \mathfrak{z} ss. *M. et sig.*—Give at once. Give tinct. opii gtt. l., and repeat if necessary.

July 10: Feels much easier. Some six or eight ounces of blood were removed by the cups. Still has much pain. Bowels opened three times. *Ordered*, Apply dry cups around the lower part of the spine and abdomen, and give tinct. opii gtt. xl.

July 11: Sitting up. Has no pain. Some soreness about the lower part of the abdomen. Her trouble for this time is over.

July 28: Her menstrual period has again occurred, but this time the symptoms are all greatly modified. Neither ovary is at all enlarged, nor is the abdomen swollen. *Ordered*, Apply a few cups and draw blood. Give tinct. opii gtt. xl.

July 29: Feels easy. Has no pain.

In a few days after her last attack, she left the Infirmary. She has not been heard from since, but in all probability she has been relieved from all further trouble by the total disappearance of the menstrual function.

CASE II.—*Ulceration and Induration of the "Cervix Uteri" with Prolapsus.*

Negro woman; named Minerva; aged 37; abode, Amelia county; employment, house servant; previous health, bad; habits good; admitted Aug. 21, 1860.

History.—This woman is the mother of two children, the youngest ten months old. Since the birth of the last child she has suffered from prolapsus. Has in her now a large globe pessary.

Report.—Aug. 21: The pessary was removed to-day and upon examination she is found to have ulceration and induration of the neck of the uterus, together with some vaginal ulceration, all of which is doubtlessly caused by the presence

of the pessary acting here as a foreign body would in any other cavity. She walks or moves about with great difficulty. The parts are very sore and irritable, causing much pain when handled or examined. She is very much emaciated, and her general health is bad. *Ordered*, R.—Cod liver oil ʒss.; Tinct. ferri chlor. gtt. xx. *M. et sig.*—Ter in die. Inject the vagina once daily with R.—Argt. nit. gr. ii; Aq. font. ʒj. *M.*—Give good diet and keep her in the horizontal position.

Aug. 30: Has been in the Infirmary only ten days, and her condition is in every respect very much improved. She can now walk about with perfect ease and comfort, a thing which she has not been able to do since the birth of her last child.

Her master is anxious to take her to the country, and she is therefore discharged to-day, without being entirely relieved, though sufficiently well to dispense with the use of a pessary.

CASE III.—*A Case of Prolapsus Uteri.*

Negro woman; named Debora; aged 25; abode, Hanover county; employment, field-hand; previous health and habits, good; admitted June 6, 1860.

History.—This woman, the mother of three children, has always been healthy and hearty, and accustomed to the ordinary work of a “field-hand.” About three weeks ago, without any known cause, a prolapsus of the uterus occurred, amounting almost to “procidencia.” She was immediately placed under the care of a physician, who partially succeeded in reducing the uterus to its natural position.

Report.—June 6: Has great tenderness in the lower part of her abdomen. Walks with pain and difficulty. The uterus is much lower than natural, and is abnormally heavy. The “cervix” is enlarged and congested, and there is also a slight abrasion on the “posterior labia.” *Ordered*, Touch the abraded surface with argt. nit., and keep perfectly quiet in bed.

June 8: She has not been allowed to leave her bed since entering the Infirmary. There is less congestion and irritation about the parts, and the uterus appears to be rather

higher than when first examined. The ulcerated or abraded spot still remains. *Ordered*, Re-apply the argt. nit., and inject twice daily with a solution made of zinc. sulph. gr. x. to aq. font. 3j. *M.*—Give tinct. cinchon. comp. 3j. three times daily.

July 11: The above treatment, together with good diet and attention to the bowels, &c., has been continued up to to-day. Her general condition has improved very much, and with it there has been a corresponding improvement in her disease. The uterus, though not entirely returned to its normal position, is sufficiently reduced to render her easy and comfortable, without the use of a pessary. All enlargement and ulceration have disappeared. To-day she was discharged with the direction to wear a compress in the perineum, retained by a T bandage.

CASE IV.—*Anteversion of the Uterus with Lengthening and Hypertrophy of the Anterior Labium of the "Os Tincæ."*
Treated at the Medical College Infirmary by Dr. J. B. M'CAW, Attending Physician.

A negro woman; named Annie; aged 26; abode, Richmond; employment, cook; previous health and habits, bad; admitted Aug. 29, 1860.

History.—This woman tells us that about three weeks ago while endeavoring to lift to her shoulder a log of wood, she suddenly "felt something give way." At the moment she had no pain, but in a few hours she began to suffer so much that she had to take her bed. There was much pain in the lower part of the abdomen, together with complete retention of the urine. In this condition she was brought to the Infirmary.

Report.—Aug. 29: Suffering extremely, unable to walk. Her system is in a weak and anæmic condition. Bowels constipated; appetite bad; tongue coated. There is anteversion of the uterus, with elongation and hypertrophy of its anterior lip, which forms a large indurated tumor that projects and completely conceals the "os" and posterior lip. She passes her urine with difficulty and in small quantities, requiring oc-

casionally the use of the catheter to relieve her. *Ordered*, R.—Pilul. hydrarg. gr. v., to be given at night. R.—Tinct. ferri chlor. gtt. xx.; cod-liver oil ʒss. *M. et sig.*—Ter in die, and keep her perfectly quiet in bed.

Aug. 30: Bowels have been operated upon twice this morning. Feels more comfortable than she did yesterday. *Ordered*, Treatment to be continued.

Sept. 1: Improving; not so much congestion and irritation about the parts. There is some abrasion or ulceration around the margins of the “os uteri.” Tongue clean; appetite good, and pulse better. *Ordered*, Touch the ulcerated spots with the stick argt. nit., and inject the vagina twice daily with R.—Zinci sulph. grs. v., aq. font. ʒj. *M.*

Sept. 4: Her general condition is very much improved. Is stronger and brighter. There still remains anteversion of the uterus, with hypertrophy of the anterior lip. *Ordered*, Paint the hypertrophied lip with R.—Potassæ iod. gr. xx; iodine gr. xx; aq. font. ʒj. *M.*—Apply a T bandage, with a compress immediately above the pubic bone.

Sept. 8: Up walking about the room. Feels easy and comfortable. Continue the previous treatment.

Sept. 14: Though not entirely well, this woman was discharged to-day, at her master's request. The enlargement and induration of the anterior lip have almost disappeared. There still is some anteversion, but not sufficient to cause her any present inconvenience. With well-adjusted pressure above the pubic bones, by means of a pad and T bandages, she feels perfectly comfortable. And in standing and walking about, as she has been for the past week or ten days, she feels not the slightest inconvenience. She is discharged with the direction to return if she has any farther trouble, that a ring pessary may be introduced.

REMARKS.—The attending physician pointed out the advantages of the Meigs' pessary in such cases as this, as being the one best adapted to keep the organ in situ, and at the same time avoiding pressure on the elongated lip of the womb,

which, with the usual instrument, would inevitably soon become ulcerated and add greatly to the sufferings of the patient; and hence, he advised this case, if any further difficulty should occur, to return to the institution so as to have the watch-spring pessary applied.

Dr. M'Caw, the attending physician, remarked, that while in cases of prolapsus and procidentia, occurring in ladies and persons leading an inactive life, whose occupation and habits called for very little, if any, physical exertion, the pessary may be used without giving rise to much inconvenience. In persons of opposite conditions in life, who have to undergo a great deal of fatigue and bodily exercise in performing their daily duties, the pessary cannot be used without giving rise to leucorrhœa, ulcerations on the neck of the uterus and in the vaginal mucous membrane, irritation of the bladder, obstinate constipation, &c., &c. If a lady will submit to the regular withdrawal and re-introduction of this instrument at short intervals; if, for instance, she has the pessary withdrawn every night and re-applied in the morning, using some detergent and cleansing injection, then the pessary may often give great comfort. Whereas, in the laboring class, as a general rule, its use will rarely terminate in a satisfactory result. Most of the cases (and they are quite a number) of this kind, brought here for treatment, are among negroes, about whom there is always some fault of the system at large, a want of tone. They are ill-conditioned and anæmic, with bad health and loose habits. It has always been found sufficient to renew the tone and vigor of the system by tonic and good diet, and to observe perfect quiet, using in the meantime some simple alterative and astringent injection.

ORIGINAL TRANSLATIONS.

I. *Diphtheritic Paralysis*. Translated from the Journal des Connaissances Médicales of July 10, 1860.

THOUGH we have already spoken of this form of paralysis, yet the importance of the subject induces us to extract from the Moniteur des Sciences the following review of an important monograph on the subject recently published by Dr. Maingault.

.....About the middle of the eighteenth century, Chomel, dean of the Faculty of Medicine, wrote as follows, in a dissertation on gangrenous disease of the throat: "My patient, who was affected with membranous angina, was not fairly out of danger till the forty-fifth day of the disease; she had difficulty in making herself understood, her voice had a nasal sound, and the uvula was elongated." And further on he adds, that a young lady who had been attacked by the same disease, on the fortieth day of the affection, spoke strongly through the nose, and had become squint-eyed and deformed; but, as her strength returned, she was restored gradually to the natural condition. Ghisi, a physician of Cremona, who wrote the history of the epidemic of 1749, speaking of an attack of membranous angina, which his own son had sustained, says that "a month after recovering from the angina, the child continued to speak through the nose, and its food, instead of following the course of the œsophagus, was rejected often through the nostrils." Thus did these two celebrated physicians sketch the principal symptoms of diphtheritic paralysis, without suspecting its nature, and without giving any further attention to it. They speak of a nasal voice, difficulty in swallowing, strabismus, and an unnatural attitude of the

body. The physicians who followed them suspected still less the existence of a new species of paralysis, and so the observations of Chomel and Ghisi were lost to science. Meanwhile, at irregular intervals, some good observer would mention a new case of membranous angina with paralytic symptoms; but these cases not being compared with others analogous to them, were unfortunately passed by in silence and neglect. What wonder? Such symptoms as those mentioned by the dean of the Faculty of Paris, and the physician of Cremona, might be often described without the least profit to medical science, if they were not arranged properly, and called by their true name—paralysis, and attributed to their true cause—diphtheria. This has recently been done by Dr. Maingault. Nothing absolutely new is ever observed in medicine; we have only better and better observed facts; and if the nosology is daily enriched, it is, happily for humanity, not owing to the appearance of new maladies. Thus diphtheritic paralysis is not a complication of morbid conditions which has only lately made its appearance, but having probably become more frequent in its occurrence, it is better understood in our day than it was formerly: once it was passed by without being seen, now no one can mistake it. The progress of science in this direction is due chiefly to the teachings of Professor Trousseau, and to the labors of Dr. Maingault.

Diphtheritic paralysis presents itself in two forms; in some cases, and those the most frequent, it is local, being limited to the veil of the palate, and to the pharynx, that is, to the region primarily affected by the diphtheria; in others it becomes general, and invades many parts of the body at once. M. Maingault, in his inaugural thesis—his earlier work on the subject—had already carefully described the local paralysis of the veil of the palate, and of the pharynx; and had pointed out the functional disturbances, and the physical signs which characterize it. In the book now under consideration the author refers particularly to the nasal character of the voice, to the pain occasioned by swallowing, to the difficulty or impossibility of distending the cheeks, of blowing through the

mouth, and of gargling the throat ; and he informs us, that on examining the throat, the veil of the palate was observed to be immovable and elongated, there were anæsthesia, and loss of special sensibility in the organ. One of the strangest things in this strange malady is, that in most cases, these phenomena continue to exist for some time after the angina is cured, when the pain has entirely disappeared, when deglutition has become easy, and convalescence is apparently well established.

M. Maingault has not satisfied himself with a clinical description of the disease ; he investigates it as a physiologist, and from the functional disturbances produced by paralysis of the veil of the palate, he obtains information with regard to the mechanism of the voice, of deglutition, and of suction.

“ When the palatine veil is paralyzed,” M. Maingault remarks, “ this musculo-membranous partition hangs inert and immovable, and does not rise to allow a free passage to the air through the mouth. The air, therefore, finding an easy course towards the nasal fossæ, penetrates them freely, causes them to vibrate, and thus produces the nasal sound.”

The rejection of fluids through the nose when the veil and pillars of the palate are paralyzed, proves, likewise, that in the second stage of deglutition these organs, by contracting, shut off the upper or respiratory portion of the passage, and separate for a moment the posterior nares from the isthmus of the fauces. The passage of the food from the mouth into the pharynx is not due then to atmospheric pressure, as held by M. Maissiat ; for on this supposition the second part of the act of deglutition would be rendered easier by paralysis of the veil of the palate ; as it is not. It is well known, that in the act of suction the mouth is the body of a pump, the piston of which is the tongue. Here, too, the active intervention of the veil of the palate is necessary ; so when this part is paralyzed, suction becomes impossible ; a smoker, *e. g.*, cannot inhale the smoke of his pipe. It is not, then, as M. Maissiat again supposes, by the atmosphere pressing on the posterior surface of the veil of the palate through the posterior nares, that the veil is lowered ; but it is by its own contraction, as M. Debron taught.

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When diphtheritic paralysis becomes *general*, in some cases the symptoms of palato-pharyngean paralysis exist in a slight degree for some days; in others, the nasal voice, and the difficulty of deglutition, have already ceased, when the muscular weakness and the disturbances of sensation manifest themselves. In some patients, we remark with surprise, the extreme emaciation which comes on, and which is not connected either with the supply of food that they receive, or with the termination of the angina. Convalescence is not well established; and very soon paralysis occurs, affecting either simultaneously or separately, both motion and sensation. The sight is enfeebled, and complete blindness may occur; but double vision and dimness are more frequent. Gradually the strength diminishes; unpleasant tingling sensations are felt in the lower extremities, accompanied with articular pains; the limbs become weaker; walking is difficult, and finally even standing is impossible. The paralysis is then *complete*. These disturbances of the faculties of sensation and motion extend to the upper extremities; the movements of the arms and fingers lose their force and precision. The head, too heavy to be sustained, falls forward on the breast, or hangs backward; the muscles of the trunk no longer support the weight of the body. Strabismus is observed, the face is distorted, the lips are pendent and allow the saliva to flow out; there is a trembling motion of the tongue, and the voice is feeble and stammering: finally the bladder and rectum may become paralyzed, and, in some cases there is complete anaphrodisia. The state of the circulation is remarkably modified; there is perfect apyrexia, the pulse, small and feeble, may be reduced to fifty beats in the adult; the action of the heart is tumultuous, and bellows murmurs are heard in the large vessels, resulting from the impoverished condition of the blood. Sometimes a little œdema is observed; sometimes, though rarely, anasarca. The skin is pale and cadaverous, and there is frequently a sensation of chilliness. Some patients cough and expectorate a ropy and fetid mucus. The appetite is often unimpaired and digestion easy; but sometimes, on the other hand, there is

perfect anorexia. Constipation is frequently observed. In the midst of all these disordered conditions the intellect, though somewhat sluggish, retains its integrity. The disease may make constant and rapid progress, and terminate in death; if, however, the result is to be favorable, the strength returns gradually, and the cure is completed in from two to eight months.

Diphtheritic paralysis occasionally assumes the form of paraplegia, but, in most cases, it affects nearly all the muscles of the trunk. The two sides of the body are ordinarily unequally paralyzed; when the case terminates favorably, the parts last affected are first restored to health. The paralysis of the veil of the palate forms an exception, however, to this rule.

Having drawn this accurate picture of diphtheritic paralysis, Dr. Maingault takes up one by one the different symptoms of the affection, and points out their relative frequency and their prognostic value. Important details on this subject may be found in his book.

The treatment is in accordance with the supposed nature of the disease. Every thing seems to show that it is an atonic paralysis; and, therefore, a tonic plan of treatment is the only one indicated, and the only one that has been employed. The preparations of iron and of quinine, together with sulphurous and saline baths, cold affusions and stimulating frictions, have been used with marked benefit. In many cases, the special excitants of the nervous system—strychnine and nux vomica—have appeared to have a salutary effect; and finally, undoubted benefit has resulted from electricity.

Dr. Maingault's conclusions are the results of an analysis of ninety cases; it may be judged from this very considerable number, says the author, that diphtheritic paralysis is far from being rare. At the present time, it is certainly very frequent, and in this very circumstance, we have a proof that diphtheria has assumed a new form. If it is true, that the chief symptoms of diphtheritic paralysis were formerly known, it is yet equally true that they were of rare occurrence. The nature of diphtheria has evidently been so modified as to assume, in

our day, the strangulatory and toxic form; its symptoms are now different from what they formerly were, and in the paralysis, we see traces of the malignity of the disease after its elimination from the system. This formidable malady, even when it is not potent enough to cause death, may yet occasion profound injury of one of the most important functions of the system—that of innervation. Thus we think it can be understood why authors as learned and acute as MM. Bretonneau, Guersant, Blache and others, who saw only the strangulatory form of diphtheria in the commencement of this century, have not described the paralytic accidents occasioned by the malignant form of the malady, which form, we repeat, was much rarer formerly than it is now.

S. C. C.

SELECTIONS.

I. *Lectures on Experimental Pathology*, delivered at the College of France. By M. CLAUDE BERNARD, Member of the French Institute; Professor of General Physiology at the Faculty of Sciences.

ON THE ANALOGY BETWEEN THE ACTION OF MORBID CAUSES,
AND THAT OF POISONS.

Gentlemen,—The general notions which, up to the present moment, have been the fundamental support of all our studies, are entirely derived from the parallel established at the very commencement of these lectures between physiology and pathology. We have shown you that physiology having, especially in these latter times, made rapid strides in advance of its elder sister, is now enabled to return the assistance which, at an earlier period, it received from the science of disease. We have, subsequently, examined the influence exerted by various agents upon the organs and tissues of the living body; a less extensive and better defined subject now lies open to our

investigation ; it is time that we should enter into the special domain of pathology, and commence the study of morbid causes. The method of inquiry which up to this point has been our guide, must still continue to direct our researches. To ascertain the mechanism of natural phenomena, and the mode of action of natural forces,—to deduce from the knowledge of facts the practical consequences which belong to our subject,—such must always be our aim, without attempting to solve the abstruse difficulties, which lie beyond the pale of human knowledge. In our introductory lecture we exhibited the regular evolution which, step by step, carries a science from its very cradle up to the highest stage of development, and you have seen that, in every possible branch of study, the progress of the human mind is arrested at a given limit. “When we arrive at a certain point,” says Lord Bacon, “Nature grows deaf, and answers no longer our inquiries.”

The ignorance of medical men in these respects has often been the object of ironical reflections ; and you well know the sarcastic reply with which Molière answers the question :—“*Cur opium facit dormire ?*”—“*Quia inest in eo Virtus dormitiva.*” But, in order to do justice to the sciences, and those who cultivate them, a question must never be put in such terms ; each separate branch of natural philosophy might in the same manner be tried and condemned. If, for instance, it were inquired for what motive all ponderous bodies fall towards the centre of the earth, we should be equally unable to answer the question. The law which Newton established has taught us in what manner the force of gravitation acts, without throwing any light upon the intimate nature of that power. It is, therefore, as irrational as unjust to blame the biological sciences for a defect inherent to all human knowledge. Let us be satisfied with inquiring how diseases are produced, how medicines cure them, and how poisons destroy life. It has not been allotted to us to know why things are ; but only in what manner they exist.

Our purpose is, therefore, to examine, in the natural order of succession, the phenomena which take place within the living body, when foreign substances have been ingested, in order to learn the mode of action which enables it to produce baneful or salutary effects (as the case may be) throughout the whole system. We have found, for instance, that woorara paralyzes all the motor nerves, puts a stop to all motion, suspends the act of respiration, and thus brings on suffocation. The practical result is, that by artificially insufflating air into the lungs during a sufficient length of time, the animal is kept alive till the poison has been eliminated, and all danger is

past; now, the same takes place with all poisonous substances which do not produce actual disorganizations of the tissues: strychnia is in the same case as the former poison; for, if all the external excitement which perpetually provokes reflex action, and thereby brings on convulsions, which end in death, if all these irritating causes, which inevitably act upon the animal left in the open air, are cautiously removed, all danger is avoided, and the animal slowly recovers, if the dose of poison has not been too large. Place a frog under a glass bell, in a cellar, after poisoning it with the alkaloid, if left there during a sufficient time, the animal is found to recover perfectly; while another frog, *cæteris paribus*, rapidly dies in violent convulsions, when repeatedly excited. The effects of cold may also be brought forward as an instance; when moderate, they benumb, but do not freeze the tissues; a frog's heart may be brought to stop under the influence of cold; but give it a more favorable medium, and it will recommence beating; if frozen, however, there is an end of all vital properties, its tissue having been disorganized.

Such is the process of reasoning brought to bear upon physiological questions; an entirely similar method ought to be followed in clinical researches, for nothing resembles so much the action of poisons as that of morbid causes of an ordinary description: the analogy holds good even in those cases in which the disorder is wholly local, and does not end in death. Corvisart, in his celebrated work on Diseases of the Heart, relates the case of a young girl, who, in order to commit suicide, swallowed a very large dose of arsenic; some symptoms of poisoning followed, but the patient recovered, and, several months later, died of consumption. In making the autopsy, a large pseudo-membranous cyst was found in the stomach; within this cyst was enclosed a solid mass of arsenic, which might have sufficed to poison nine or ten persons: this was the residue of the primitive dose, a very small portion of which had been absorbed, while the remaining part, incased in this accidental cavity, rested within the stomach without producing other effects than those of an ordinary and inoffensive tumor. Now, if the cyst had been a spontaneous morbid production, instead of resulting from the ingestion of a foreign substance, the consequences would have been identically the same, unless inflammation, suppuration, and resorption had taken place within the tumor.

If we examine some of the diseases which most frequently produce death, we shall equally be obliged to have recourse to general effects, in order to explain the mechanism through which the ultimate result is attained: numberless patients die

of peritonitis, and in a very short space of time too; how does this take place? for peritonitis, at first sight, does not seem to interfere with any of the higher functions of life. Inflammation of the lungs or pleura frequently proves mortal in a few days; and in such diseases, the respiratory functions are, of course, impeded: yet mere asphyxia is evidently not the cause of death in acute cases of pneumonia; and in affections which rapidly prove fatal, the animal, although deprived of food, cannot evidently be supposed to die from mere inanition in so short a space of time. It, therefore, becomes necessary to proceed to a rational investigation of *all* the diseased tissues, in order to ascertain the mechanism through which death has been produced: both nerves, muscles, glands, and other tissues: both the solids and liquids of the body require to be examined. If, for instance, the substance of the liver is submitted to chemical analysis, it is found to contain no more glycogenic principles; the total disappearance of which is, in our opinion, one of the most ordinary causes of death; for animals kept fasting for several days together still retain a certain amount of sugar in the blood. It, therefore, seems that life may be extinguished in two different ways: firstly, by the introduction of deleterious principles in the blood; and, secondly, by the total absence of indispensable elements in that fluid. From such instances, it is hardly difficult to judge what degree of scientific accuracy we may expect to find, in ordinary post-mortem examinations: local lesions are exclusively sought for, while the general disturbance passes unperceived; and, even supposing its existence to have been suspected during life, how difficult it becomes, in the human subject, to ascertain the fact after death! Twenty-four hours must have elapsed before we are allowed to touch the corpse: now, although in animals recently slain the natural properties of healthy tissues persist, during a certain space of time, we are perfectly aware that, after a few hours, they are no longer to be found; such, for instance, is the case with respect to the galvanic excitability of muscles and nerves in birds and mammiferous animals. If, therefore, the effects of woorara, digitalis, and other poisons, which act upon these very tissues, had been exclusively studied in the human species, we should never have been able to ascertain by comparison the precise nature of the injury.

To observe Nature in her operations is the only method of acquiring the power of turning them to our advantage. The extraordinary progress of science and industry in the course of the present century, and the astonishing forces which have in consequence been placed at our disposal, have given rise to several false notions, which, however, are at the present mo-

ment currently received; and we frequently hear it asserted that man holds the elements under his control; that fire, water, steam, electricity, and all the powers of Nature, obey his commands. Now the exact reverse of these opinions is truth; and, as Bacon philosophically observes, "*Naturæ non nisi parendo imperatur*;" we are, in reality, only submitting to Nature at the very moment when she appears to obey our will. A singular phenomenon, of very unusual occurrence in our climate, has lately afforded us a striking instance of this. During the aurora borealis which took place in Paris in the course of last summer, strange and inexplicable perturbations were noticed in the action of the electric telegraph; the wires no longer conveyed the electric fluid to its ultimate destination, and, for a short space of time, intelligence could no longer be communicated to Paris. The impossibility of controlling Nature was in this case placed in a very strong light; had we been acquainted with the law which directed the unusual effects of that unknown cause, we might have obviated the inconvenience which our ignorance compelled us to undergo. The situation in which medical men are placed, in the treatment of diseases, is exactly the same: to alter their course is beyond our power, but the accurate observation of their successive symptoms sometimes enables us to interfere at the proper moment.

It is, therefore, altogether indispensable to combine experimental researches with clinical observation; to create artificial disease by known means in living animals, and proceed, immediately after death, to a rational post-mortem examination; all the tissues must successively be compared to those in a normal condition, but the state of the blood more especially deserves our attention. Towards this subject the energies of all physiologists ought to be mainly directed. Organic chemistry, however, is unfortunately not in an advanced state, especially as regards the constituent principles of the animal organization; and chemical analysis must, therefore, be in a great measure left aside when the properties of the blood are the subject of investigation; in other terms, a physiological analysis is, in this respect, far preferable. Let the vital properties of the nervous system be brought into play, and modifications of the very highest importance will be discovered, which neither morbid anatomy, nor the known methods of analysis, would have been able to render apparent. The same is the case with muscles—when poisoned by certain substances, they lost their contractile power, but no modification whatever is to be discovered in their chemical constitution. The blood, therefore, may naturally be expected, in various states of health and dis-

ease, to present changes which none but the physiologist can appreciate; thus, when oxide of carbon has been inhaled, the blood-globule is deprived of its characteristic property of absorbing oxygen in exchange for carbonic acid: the fact is ascertained by physiological experiments, but ordinary analysis gives no clue whatever to the solution of the problem. Far be it from me to disapprove the chemical researches of which the blood has been made the subject; on the contrary, they deserve to be carried out on a more extensive scale than ever; but if performed apart from experiments on the living animal, their results will never contribute to the real progress of science.

The study of morbid principles can, therefore, alone enable us to discover the means of curing the disorders to which they give rise; and for this purpose two different systems might be adopted; firstly, to neutralize the morbid agents; and secondly, to eliminate them from the body. In the present state of our knowledge, we possess no means whatever of neutralizing their actions; as in the case of poisons, they must be expelled before they cease to act. To this result do all the efforts of Nature tend, and to this result also must all the physician's endeavors be devoted.

II.—*Extracts from an Essay on Diphtheria and Diphtheritic Affections.* By A. JACOBI, M. D.*

What is diphtheria?

Diphtheria, or diphtherite, is, in its broadest sense, and from a merely anatomical point of view, a morbid condition recognisable by the exudation of fibrinous pseudo-membranes. They generally coincide with pernicious forms of inflammation, and are, in the majority of cases, exuded after a catarrh has preceded. The pseudo-membraneous exudation consists of fibrine, and is generally accompanied by serous secretion in its neighborhood, and hyperæmia and swelling of the surrounding parts. The microscope shows it to be an amorphous and homogeneous mass, usually interspersed with imbedded epithelial cells, and by multiform, sometimes spherical, sometimes angular, corpuscles of mucus. The exudation is tough and elastic, or soft

* In answer to many enquiries from our readers, we take copious extracts from the excellent paper of Dr. Jacobi on Diphtherite, found in the columns of the N. Y. Medical Times. These observations are based on an experience of nearly five hundred cases, and are worthy of especial attention.—EDITORS.

and pulpy, or friable, sometimes very apt to macerate, and very much like a serous liquid. It is usually membranous, expanded over a plain surface, sometimes extending into the subjacent tissue, and in some few cases forming hard lumps, of irregular shape, of a fibrinous mass imbedded in the organic tissue. Its composition is proved to be fibrinous by chemical tests. In this respect it is like the plastic exudation into the bronchi and lung-cells observed in pneumonia, where fibrinous coagulations are sometimes expectorated together with bronchial mucus.

The pseudo-membranes differ in size, thickness and color, as well as in consistency. Their shape is very multiform; some are round, some angular, some regular, some irregular; their thickness varies from a transparent thinness to a quarter of an inch and more; their color is white, glassy, greenish, grey, yellowish, reddish, brown, according to their thickness, exposure to the air, and admixture of blood; sometimes unaltered blood adhering to its lower surface. They are either merely adherent to a mucous membrane, without any alteration of its tissue; such is usually the case on the mucous membrane of the bronchi and trachea, and mostly on the velum palati. Or they are imbedded in its substance, as mostly on the tonsils, and the posterior wall of the pharynx, and frequently in the larynx. It is also a very remarkable fact, that the same continuous membrane will alternately be readily removed from the surface of the mucous membrane, and again at a very short distance tear the substance of the mucous membrane at every attempt at separating it. The surrounding parts are hyperæmic, and swelled by œdema during life; at post-mortem examinations the œdematus swelling is sometimes found, but the hyperæmia is no longer met with after the refrigerating and contracting influence of the atmosphere has had time to operate. Pseudo-membranes are found in the pharynx, on tonsils, uvula, and velum, on the gum, lips, and tongue, on the mucous membrane of the mouth and nose, in the larynx, trachea, and bronchi, in the superior part of the œsophagus, in the lower part of the intestinal canal, round the anus, in the vagina, the external ear, the naso-lachrymal duct, on the conjunctiva, and on the cutis wherever and by whatever cause it has been deprived of its surface, sore nipples, etc. In all these places the chemical as well as the microscopical constitution of the membranous exudation is entirely the same.

It is a remarkable fact that the majority of inflammatory processes attended with membranous exudation take place in the pharynx; cases of both sporadic and epidemic pharyngeal diphtherite will sometimes be accompanied with diphtheritic pro-

cesses of other parts; but diphtherite of the external ear, or vagina, will but seldom be heard of without being accompanied with pharyngeal diphtherite. The few cases of this kind observed by us we count among exceptional curiosities. Next to pharyngeal diphtherite, in frequency, comes the mucous membrane of the respiratory organs, especially of the larynx and trachea, and nostrils. Sometimes the exudative process will either extend from one part to the adjoining one, or will merely wander; sudden transmission to distant regions, or contemporaneous affection of distant parts, being usually not observed, except in epidemics of diphtheritic affections. To this point we shall return at some future occasion.

We have stated, that the majority of diphtherites are met with in the throat. Thus it occurs that the term of diphtherite is usually applied and attributed to the peculiar process of exudation in the fauces; so much so, that we generally have forgotten that dysentery, croup, etc., consist in, and show, the same anatomical lesions as diphtheritic sore throat; the difference of accidental symptoms and our tendency to classification either preventing our perceiving or acknowledging the equality in what simulates differences. Certainly, the same physiological or pathological process, in different organs, ought to be understood to offer a series of different symptoms.

Our past epidemic has again proved that diphtherite is, in almost all cases, confined to, or originates in, the throat. Thus, diphtherite and diphtheritic sore throat are almost, as it were, identical terms. At all events, the presence of membranes in the throat may be taken as the ordinary occurrence; and in the following symptomatology we speak with particular regard to this usual form of diphtheria.

A child shows symptoms of moderate fever; pulse accelerated, perhaps a little small; face somewhat flushed, submaxillary region a little swelled to both sight and touch; more or less headache is complained of, swallowing has been interfered with for some days; tongue has a soft, creamy whitish, or whitish grey or yellow fur, diminishing in thickness towards the lateral, and particularly the anterior region, where the papillæ appear a little enlarged and higher colored. One or both tonsils, or the posterior wall of the pharynx, the velum, uvula, or some of the enumerated parts at the same time, are covered with a membrane as described above. The surrounding mucous membrane looks red, livid, is hyperæmic, and shows œdematous swelling. The submaxillary and cervical glands are a little swollen. The breath of such patients has generally a bad smell, although the membrane does not extend very far; at all events, some smell will be perceived as soon

as the membrane begins to macerate or fall off; especially is this the case when the throat is not kept clean. Such are the average of mild cases, and the great majority of all those which come under observation in the onset of an epidemic. They will get well in five, six or eight days; some spontaneously, some after appropriate treatment. The membrane will be removed as a whole, and leave the mucous membrane underneath smooth, livid; or it will macerate, fall off in pieces, or be removed as a half-serous liquid, and leave the mucous membrane, especially of the tonsils, with a ragged appearance and superficial ulcerations. At the same time, the submaxillary and cervical glands will return to their normal size.

A number of cases occurred during the epidemic, in which all the symptoms were present, with the exception of the formation of the membrane; there were some amongst them in which the symptoms of fever and adenitis, and even a general adynamic condition, were well pronounced. In such cases we never thought of putting the case down as diphtheria, but recorded it as pharyngitis, amygdalitis, or stomatitis; a number of cases which we, according to the severity of symptoms, supposed to be of a diphtheritic nature, we put down as diphtheritic pharyngitis, or diphtheritic fever; the former name being selected for such cases in which the symptoms of pharyngitis prevailed, the latter for such as showed fever as their foremost symptom. We were seldom mistaken, as in a day or two membranes would generally present themselves.

There is a form of the diphtheritic process, in which very little or no fever is perceived, and little or no glandular swelling will take place. The congestion and swelling of the pharynx is not very remarkable, and the first remarkable appearance is noticed on the follicles of the mucous membrane of the pharynx: they are visible as whitish grey spots of a twentieth or twelfth of an inch in diameter. Not long after, however, membranes are formed, and the whole process will run its course in sometimes three or four days, without any great inconvenience to the patient. But there are cases in which the symptoms will increase in severity, fever will set in, and submaxillary and cervical adenitis take place. Such cases have been separated by some authors as "common membranous angina," as "herpes of the throat," as "herpetic angina." We do not see anything else in these cases but light diphtherite, mostly without well pronounced general symptoms. We have not found any more reason to distinguish this form, of which, however, we have not seen more than a dozen of cases, from diphtheria, than we should think of excluding a case of scarlatina, without fever, and with less than usual eruption,

from the record of cases of scarlatina. Moreover, we have pointed to the fact that such apparently simple cases will sometimes be followed by fever and adenitis; and when we further add that some of these light cases of "herpetic angina" have been followed by diphtheritic paralysis, we ought to lay aside our fondness for classification and subdivision; and take nature as a whole, the different pathological conditions of the diphtheritic process are variable in their appearance but alike in their innermost nature.

The local process of exudation is not confined to a certain limit. Membranes will be found on the posterior wall of the velum, and in the posterior nares, blocking up the nostrils and extending as far down as the lips. In such cases small children, who are mostly used to breathe through the nose, suffer much from dyspnœa. These membranes will be removed in the course of time, and with the same consequences as those of the pharynx itself. To one peculiarity, however, we wish to direct the attention of our readers. We have once seen, in consultation, a little girl of five years, in the upper part of the city, with extensive membranes of the pharynx, velum, etc., but were struck with the fact that long cylindrical tubes of false membrane were brought up. These cylinders could come from the trachea only; how was it that the child was not suffering all the time from the utmost dyspnœa, but felt proportionately not bad and gave, in its general appearance, a good prognosis? We learned from our friend, Dr. S., who was then suffering from extensive diphtheria himself, that he, too, had brought up cylindrical tubes of membranes like those thrown off in children suffering from laryngeal and tracheal diphtherite, but that in his case they came from the posterior nares. We desire to state this fact, supposing that some professional man might in some case have the same difficulty in explaining the occurrence of cylindrical tubes of membrane that do not come from the respiratory organs.

In other cases the exudation of pseudo-membrane will extend downwards, instead of upwards, and will then constitute what is generally called croup. The diagnosis of this descending croup is difficult in one respect only, viz: The oedematous swelling surrounding the membranous exudation will sometimes produce croupous cough and dyspnœa, etc., before the membrane has reached the larynx, by oedema of the epiglottis, or glottis, or general laryngeal catarrh. As a general thing, there is not much practical difference, for in the majority of such cases the process is of such rapidity, that we need not wait long for real membranous exudation. At all events, the symptoms of this descending croup of the diphtheritic process

are the same as are known in genuine or sporadic croup; and therefore, after we have learned besides, that the anatomical elements and constitution in diphtheritic membranes of the pharynx and larynx are the same, we have a right to say, that there is no real, anatomical difference between "diphtheritic" and "genuine" croup. We need not return to the consideration of the former assumption of a difference between croupous and diphtheritic membranes. It does not exist. Croupous membranes were those which were stretched over the mucous membrane, while the name of diphtheritic membranes was given to such as would be imbedded in the membrane and leave ulcerations, or loss of substance, when removed. There is no such difference.

There have certainly been cases of croup, genuine croup, differing widely from each other, in the experience of every one of our readers. The differences are either of an individual or of a general nature. The former do not interest us here, the latter do. Genuine croup is apt to cause death either by suffocation, or by poisoning with carbonic acid; and "diphtheritic" croup may show a third and fourth cause of death, viz. exhaustion, and diphtheritic poisoning. And it is here that we wish to add some remarks on the true nature of diphtheria, as shown by observation and rational conclusions.

Diphtheria is a general disease; it has local deposits, it is true, but in the same manner that scarlatina will localize itself on the skin, mucous membrane of the Bellinian canals, etc., measles on the skin, mucous membrane of the respiratory organs, etc., or typhoid fever on the mucous membrane of the intestinal tract, etc. Thus diphtheria will, being eminently a constitutional disease, localize itself on the mucous membranes and denuded skin in general, and on the mucous membrane of the pharynx and larynx in particular. Therefore, the danger of the disease depends by no means on the extension or thickness of the exudation, sometimes a very small amount of exudation taking place in extremely dangerous cases. Some cases have set in with an exceedingly high fever; with vomiting, which so generally is a symptom of sudden and general affection of the system; with convulsions, like those observed at the outbreak of acute exanthems, and with rapid collapse not at all explained by any of the visible symptoms.

Although not all the fatal cases ran their course with this entire absence of positive symptoms, terminating in death as it were without disease, a great number of cases have come under observation, in which the local exudations were by no means in proportion to the general symptoms and to the character of the attack. Excessive fever, with a pulse of 140, 150, 160, like

that in scarlet fever, but generally of a more adynamic character, and large swellings of the submaxillary and cervical glands have in a large number of cases been observed to precede the exudation of fibrinous membrane for three, four, even five days. Then, at last, membranes would make their appearance, and either cover, in some instances, the pharyngeal mucous membrane and the adjoining parts to a large extent, or, in most cases, remain for a time of moderate size. Such cases are never without danger; the adynamic character of the disease, combined with the intrinsic danger of a fever like that described, being fully able to produce death from exhaustion in a very short time indeed. Such have been those cases also in which a septic character would develop itself. Instead of being removed as a whole, or in pieces, the membranes would decay, macerate, or flow off, with the serous secretion of the adjoining mucous membranes, as a thin sanious matter, of watery color, and excessively foul smell, and would exulcerate every healthy part it came in contact with. Ulcerations, on the original seat of the membranes, or such as were successively formed on neighboring localities by contact with the sanious, foul secretion, would again prove the seat of new secretion and stench, and sometimes require many weeks, even months, in such individuals who suffered most from anæmia and exhaustion, to perfectly heal. We will here refer particularly to those cases of which a large number have been observed. In a family where diphtheria was just reaping its harvest, or in the neighborhood, children would fall sick with all the initial symptoms of diphtheria, excessive fever, exhaustion, pharyngitis, and adenitis, but no membranes would appear. The whole would take a course like that of diphtheria, and the patients require a long time to recover. Such cases we have always put down as diphtheritic fever. We feel sure, however, that they were entitled to be named diphtheria like those in which membranes were formed. Similar cases will occur in other epidemics, where part of the symptoms only will be fully developed, without however losing their general character; there is a great difference, too, among cases of scarlatina in every epidemic, according to individual nature, accompanying circumstances, and the severity of original infection or contagion. Further, there is a great difference depending on whether a case of zymotic disease will occur sporadically, or during the height of an epidemic; sporadic cases, and such as occur at the commencement of an epidemic, generally proving to be milder forms. Thus diphtheria, in sporadic cases, or in the beginning of the epidemic, may look like a merely local disease, being in its nature a general affection of the system. Diphtheria with

its local exudation into the larynx has, for many decenniums, generally been observed sporadic, and therefore has been taken to be a merely local affection. It has been otherwise in our epidemic. At all events we believe we have shown that pharyngeal as well as laryngeal diphtheria may have the appearance of either a local alteration or a constitutional disease; its true nature being general and constitutional, like scarlet fever, or any of the diseases comprehended under the head of zymotic affections. At this point, finally, we allude to the real meaning of the terms of "diphtherite" and "diphtheria." According to its origin the former means a local, the latter a general affection. If a uniform name was to be given to all the cases occurring, both sporadically and epidemically, it could be that of diphtheria only; but such cases as exhibit more of a local character, might as well be called pharyngeal, laryngeal, cutaneous, etc., diphtherite.

We have a few more words to say on the swelling of the submaxillary and cervical glands. It is a symptom that will be found in almost every case, and never missed in any severe one. It is so certain to be met with, and so little, in many cases, in proportion to the extension of the membranes, and, moreover, so generally keeping pace with the danger of the fever, that we have been taught to consider the amount of glandular swelling, accompanying *pharyngeal* diphtheria, as a highly valuable prognostic symptom. It will often remain after the other symptoms have subsided, but the case must not be considered to be perfectly safe as long as the glandular swelling or induration lasts. Suppuration of the diseased glands we have met with in but very few cases. We consider the glandular swelling as a means of determining the amount of constitutional infection.

The epidemic nature of the disease is an acknowledged fact; consequently we do not wish to carry owls to Athens by repeating what every reader knows. Nor is it necessary to say much on the communicability of the disease by endemic influences. For every practitioner who has but seen a limited number of cases, has certainly learned, that cases will seldom occur isolated in one locality. Generally, a number of inhabitants of the same room, house, or neighborhood, will be affected at the same time, or in short intervals. Thus hospital wards containing cases of diphtheria, will soon exhibit a number of other cases of the same disease. This fact has been so often reported, and is so well known, that this source of infection need not be dwelt upon any longer. But the question arises, whether many cases believed to be the result of the same general endemic influences, exercising their power on every inhabitant

alike, have not rather been the consequences of direct contagion. This question of the contagiousness of diphtheria has been the stumbling block of most writers, and we must confess, of some of the most conscientious ones. For certainly the more scrupulous an author would be, the more he will feel bound to restrict the territory of contagion in all cases that give a chance of being explained by endemic influences alone. For our own part, we at once state our conviction of the contagiousness of diphtheria. We know full well that the proof thereof is not easy. Inoculation has proved either fruitless or improbable; the cases of surgeons related to have been directly affected by diphtheritic patients, admit of other explanations; the vast majority of cases believed to be, and reported as proofs of contagiousness, are really better explained by local endemic influences.

* * * * * We do not mean to say that it necessarily must be communicated to everybody coming in contact with a patient—although we have repeatedly observed such members of a family as were most engaged in waiting upon those already affected, to be the next sufferers—just as little as we assert that the like endemic cause existing in a room, or house, or neighborhood, must necessarily strike down every human being under its influence. Nor do we think, that no case of diphtheria could originate in any other way except by contagion. But we feel sure, that it partakes of the nature of such epidemic diseases as typhoid fever, or scarlatina, or measles. The failure of experiments is no proof; for when has the direct experiment on the contagiousness of typhus, scarlatina and measles, or on their inoculability, given any satisfactory result? And nevertheless, although there are some who deny contagion by typhoid fever, all three are recognized to be propagated by direct communication from one individual to another. The diphtheritic membranes, moreover, observed to form on the sore nipples of mothers or wet-nurses, whose children suffered from diphtheria, appear to be as good a proof that contagion is a cause of diphtheria, as any that could be found in any other disease.

The complications of diphtheria are very numerous. Nothing else can be expected in a disease, which is as apt to last a long time as it will generally impair health and nutrition for a protracted period. We do not count coryza, croup, etc., mere localizations of the disease, amongst its complications, nor shall we have any thing to say on hæmorrhages occurring during its course until we enumerate its final consequences; nor do we feel sure whether we are right in considering pneumonia with its rapid exudation as a mere accidental complica-

tion. For it is certain, that pneumonia will very often occur during the diphtheritic process, and may easily be influenced by the peculiar constitution of the blood giving rise to rapid and copious fibrinous exudation. It is well known to all of our readers that croup will oftentimes be complicated with pneumonia, and even prove fatal not by its own influence, but by the sudden and extensive propagation of the exudative process to the bronchi and lungs. Whichever opinion may be right, it is certain that pneumonia is very frequently met with in company with diphtheria. We have further noticed as complications, of more or less accidental nature, several zymotic diseases, as scarlatina, measles, urticaria, furunculosis, erysipelas, intermittens, and intermittent cephalalgia, varioloid and acute rheumatism; further, general anemia, paralysis (pre-existing), rachitis, pulmonary tubercles, eczema of the face, dysuria, stomatitis aphthosa and ulcerativa, gangrene of the throat and noma; gastric, intestinal and gastro-intestinal catarrh, laryngeal catarrh; dilatation and hypertrophy of the heart; hyperæmia of the brain and meningeal œdema. Many of these complications are very dangerous and frightful indeed. We have attended a young man of nineteen years, residing in Chrystie Street, in whom the pharynx and nostrils were the seat of diphtheritic membranes; adenitis and fever being in the beginning very moderate. The membranes were hard and thick, and proved very obstinate for some weeks until, in February, the patient was affected with varioloid. From this time the strength of the patient gave way, the fever rose, appetite disappeared, mucous membranes grew pale, skin very inactive. The usual time of desquamation passed by, but no desquamation took place; the scurfs grew hard and dry, and gave a great deal of pain on being touched; at the same time, they increased in height and diameter, so as to urge upon us the necessity of removing them by force. Beneath each of them the surface was a deep discolored ulcer, secreting a whitish yellow or greenish pale matter from a whitish gray membrane that covered the ground. Thus each varioloid pustule constituted a diphtheritic ulcer, a hundred of which covered the head and face and trunk and extremities of our unfortunate patient. It may suffice to say, that he required several months before the diphtheritic ulcers of his scalp and face got well, and that he has not recovered his former strength and health up to this moment. Now, that such complications of two zymotic diseases should occur, is not at all improbable, nor are they very rare. There are many cases in which we should do well to remember, that a complication of scarlatina and measles, or of measles and urticaria, form a compound that is sometimes met with in prac-

tice, and is very apt to mislead the practitioner, and to be misconstrued by him. Nor is it an entirely extraordinary occurrence to see an acute exanthema step in before another has fully completed its course. Thus we have the case of a child, on whom we were fully able to diagnosticate, to our entire satisfaction, the presence of scarlatina, urticaria, measles, and finally varioloid, in the course of thirty-two days from the first to the last attack.

Not an unfrequent complication of diphtheria is albuminuria, which may be observed in any stage of the disease. It is not at all true, that albuminuria in diphtheria and in scarlatina shows the difference pointed out by French writers, viz: that albuminuria is a complication of the first stage of diphtheria, and of the later periods of scarlatina; this assumption being equally erroneous in either of the two. About a quarter of all the cases examined for albuminuria gave a positive result as to its presence, and nearly all the post-mortem examinations revealed an hyperæmic condition of the kidneys; this being the first cause of, or at least co-existent with, albuminuria. Thus it appears that albuminuria, although found in many cases of no severity, nor sometimes attended with any danger, is a complication of more than average importance.

The etiology of diphtheria shares the fate of that of all the zymotic diseases, of being exceedingly obscure. It is an endemic as well as epidemic disease, and will therefore be found with every constitution, amongst all classes, in either sex, at all ages, in all climates, and at all seasons. But some differences are found that modify our assertion to some extent. The first four or five months of both years, 1859 and 1860, show by far the largest number of cases. In our cases, besides, the male sex was in a slight but decided majority throughout the whole course of the epidemic. Age, too, shows differences similar to those exhibited for scarlatina and measles. The two hundred cases enumerated above are of children under fourteen years of age; the number of diphtheritic cases beyond this age, in the same institution, for the whole time, is from thirty to thirty-five; moreover the large majority of those two hundred cases occurred in children from two to five years of age, the average age being three years. Cases under a year are not frequent, over nine or ten years proportionately rare. We do not remember to have seen a patient suffering from diphtheria at the age of more than fifty years, but have been told of some few occurring between seventy and eighty. The majority of cases were children with impaired constitution, badly fed, and with bad digestion, anæmic or suffering, or convalescent from another disease that diminished the amount

of the solid elements of the blood, and the strength and power of resistance. Among the most unpropitious accompanying or preceding diseases are scarlatina and measles, which generally give a pretty unfavorable prognosis when existing with or followed by diphtheria. Scrofulous individuals, with catarrhal affections of the mucous membranes, of long standing, are particularly subject to being affected, and thus, undoubtedly, poverty, dirt, and a want of care have a great influence in producing this and other general diseases. Dense population must be accused in many instances, firstly, because of its very co-existence with poverty, want of fresh air, and uncleanness; and secondly, because of the readiness of communication by direct contagion. But it is nevertheless a fact, that many cases, and many dangerous and fatal ones, occurred in healthy and robust children, wealthy families, good situation, and fresh air.

Our knowledge of the pathological anatomy of diphtheria is very defective, especially in regard to such alterations as take place in the blood and general system. The microscopical appearance of the pseudo-membranes has been described in our remarks on the nature of the disease; *oidium albicans*, and *leptothrix buccalis* have been found by some writers, and have even been considered by them to be the cause of contagious transmission of the affection; we have not, however, been able to identify them in those few microscopical examinations we have made; and we believe that those microscopical fungi are but accidental occurrences in the exudation. We have further described the appearance of the mucous membranes as to whether the exudation was but a covering to its substance, or imbedded in its tissue and leaving more or less deep ulcerations. Pneumonia, bronchitis, and the results of complications of diphtheria were not unfrequent in those fifteen or sixteen cases where we were allowed to make a postmortem examination. The skin would sometimes present the appearance of furfuraeous desquamation on some parts of the body, as it will be found in a large number of diseases attended by high fever and rapid diminution of the subcutaneous tissue. Petechiæ were found in a few instances, generally most in the præcordial region, hypogastric region, and on the thighs. The submaxillary and cervical glands, with the surrounding tissue, were generally swelled, but not to such an extent as might be expected from the swelling during life; the tonsils were either swelled, not hyperæmic, or covered with deep ulcerations, or again consisting, to a great extent, of a grey fibrous mass; uvula ulcerated, sometimes much diminished in size; liver and spleen in a few cases greatly hyperæmic in spite of general

anæmia; in one instance there were apoplectic clots in the tissue of the spleen; in others they were as anæmic as the body in general. The blood was generally thin and liquid, and several times of a dark, scorbutic color; the meningeal blood-vessels were distended with blood, undoubtedly a consequence of moribund condition. In the case of death from uræmic convulsions, in Hoboken, a small vein near the left sinus transversus was ruptured and a small clot of blood presented itself outside the vessel. Thus the results of post-mortem examinations are certainly few; there is nothing in them characteristic of diphtheria, except the local alterations; and in this respect diphtheria takes part in the general nature and anatomical mystery of the zymotic diseases as a class.

The diagnosis of diphtheria, and particularly of diphtheritic affections of the pharynx ought never to be difficult. At all events, it can be made with less difficulty than scarlatina, which will not unfrequently be recognized from its consecutive diseases only; for there is one pathognomonic symptom which will never fail to give certainty, viz., the pseudo-membrane. All the other symptoms, as headache and earache, submaxillary and cervical adenitis, erythematous or œdematous swelling of the fauces, fever, convulsions, toxæmia, and prostration, foul smell from the mouth and nose, albuminuria and hemorrhage, severe though they may be, are not pathognomonic. Each of these symptoms may either be produced by other diseases, amygdalitis or general pharyngitis, any feverish disease, particularly scarlatina or other acute exanthems, gangrenous sore throat or meningitis; or they may be absent in a case of genuine diphtheritic affection. The difference, as to severity and danger, in diphtheritic affections is at least as great as in any other epidemic malady; and many cases have such an innocent appearance that authors have been induced to comprehend them under the name of herpetic angina. As to this we have stated our reasons for considering them as mild forms of diphtheria, which, however, may lead to very severe consequences; there is in them a local diphtherite with no fever or only a slight febrile attack with intermittent character. Nasal discharge, and foul smell from the nose and mouth are not at all required to ensure the diagnosis, nor is any symptom of the same importance as the exuded membrane. It will not be difficult to distinguish the diphtheritic membrane from gangrenous pharyngitis; but it must be well borne in mind, that local gangrene may be a complication of diphtheria. Even Bretonneau has observed, that diphtheritic membranes would sometimes cover the ulcerations produced by pultaceous cynanche. The differential diagnosis from scarlatina will, in some instances,

cause difficulty for several reasons. Scarlatina is frequently complicated with diphtheria; thus, wherever the eruption is of no account or escapes the attention of the physician, a mistake is easily made. Secondly, albuminuria is common to both, and perhaps there is no other difference between the two except this, in many instances, that the urine of scarlet fever patients will more generally show casts. And finally there is, too, an erythematous eruption of the skin which may be mistaken sometimes for the eruption of scarlatina. Some writers, like Dr. Peter, take such cases to be scarlet fever complicated with diphtheria; such an one of the two maladies as is produced by a greater epidemic influence exhibiting the principal symptoms. We feel no inclination to agree with this opinion; for such cases will sometimes turn out as diphtheria, from subsequent symptoms, as, for instance, paralysis; no scarlatinous desquamation has ever been observed by us, and the little furfureous desquamation that sometimes will take place, has, in some instances, been found where no erythematous eruption had preceded, and finally, a light erythematous eruption is not an absolutely uncommon occurrence in any of the febrile diseases of infancy. We finally add, that we have observed diphtheria in individuals, and even returning for the second time, who had suffered from scarlatina before. This remark we make in regard to those who are inclined to recognize an identity between scarlatina and diphtheria.

One case under our observation offered a remarkable point as to diagnosis. A man suffered from what appeared to us to be pharyngeal diphtheria, with fever, etc., for some days before he was taken into the Jews' Hospital; he grew anæmic and prostrated, but the membranes did not take very long to disappear from the soft palate and uvula, under a simple antidiphtheritic treatment. Something like a fortnight afterwards the uvula and soft palate again exhibited a grey coating, which soon, in the course of a few days, increased considerably in size and showed the symptoms of syphilitic ulcerations. The patient denied ever having suffered from chancre, nor could we discover any marks. Our antidiphtheritic treatment was continued for about six days until the soft palate was perforated. Feeling sure, then, that we had to deal with secondary syphilis, probably brought on prematurely by the preceding diphtheritic affection and œdematous swelling of the parts, we commenced a mercurial treatment; the patient confessed as to his having had syphilis, but it was then too late to save all of his soft palate.

The prognosis depends on the condition of the child preceding the attack, on its age and general strength, on the char-

acter of the affection, and on the absence or presence of dangerous complications. Children will die in a larger proportion than adults; these being affected in a larger number with diphtheria than with scarlatina, etc., but for no other reason except this, that scarlatina will, as a general rule, take effect once during the life of a patient, while diphtheria has a great tendency to return. Anæmic and sickly children are in great danger under all circumstances; cutaneous diphtheria will prove fatal in many instances during the first year where cutaneous eruptions are common occurrences. The extension of membranous exudation in the pharynx, etc., does not always produce a proportionate malignity, except in small children where the breathing is particularly effected through the nose, and even a slight obstruction of its cavities and moderate swelling of the pharynx is able to prevent decarbonization of the blood. Scarlatina, measles, noma, gangrenous sore-throat, large swelling of the submaxillar and cervical glands, high fever, and extensive pneumonia and bronchitis give no favorable prognosis. But the average mortality is not so great as might be inferred from the reports published on some epidemics. The cases of mild form and average severity are the large majority, and will recover with a rational treatment. Severe cases often perish in spite of any thing that may be done to relieve them. Cases that set in with a high fever, considerable adenitis, cerebral symptoms, intense head-ache and ear-ache, and convulsions, small frequent pulse, and a foul smell from the nostrils and mouth, must be considered as extremely dangerous. Hæmorrhages, at any place, and of any kind, must be considered as a severe complication, as they indicate a deep-seated alteration of the blood; we have observed hæmorrhages from the nose (right nostril) and intestines and petechiæ. They must be considered as of equal importance to those occurring during or after scarlatina, erysipelas, etc., even in some of those cases where the nasal hæmorrhage is produced, as *causa proxima*, by exulcerations of the nasal mucous membrane. Cases of average severity will take from five or six to ten and twenty days to recover; severe cases, unless they prove fatal during the first period, may take several weeks or months. But the large majority, according to our experience, ultimately recover. Of about five hundred cases, we believe we have lost not more than thirty, but we have seen very protracted convalescences in most of them, depending on the prostration of the nervous power and the anæmic condition of the patients. The causes of death are of various nature. Sometimes it is the dissolution of the blood, the exceedingly high fever, cerebral symptoms at the first onset or towards the end, exhaustion from

want of food and absence of digestive power, general nervous prostration, protracted anæmia, or suffocation from obstruction of the respiratory organs.

We need not add that the prognosis becomes more unfavorable than it will be under other circumstances, from the fact that the disease is very apt to return. A second attack in a child exhausted by a preceding one will readily prove fatal. Even laryngeal diphtheria has been observed to recur, although we have never seen an instance of croup twice occurring in the same individual. Guersant performed tracheotomy twice in each of two children, after intervals between the two attacks of eleven and of twenty-one months. A few other cases of the same kind have been reported.

One of the most remarkable consequences of diphtheria is total or partial paralysis or paresis. Paralysis of the velum palati will, in many cases, be observed during the course of the exudative process and œdematous swelling, and in this case must be taken as directly depending on the natural alteration of the tissue. The voice will have a nasal twang, and food, particularly of the liquid kind, will regurgitate through the nose, but in other instances it will not make its appearance before the process is all over, and the patient, although still anæmic, on a fair way to convalescence. We saw, this week, a little boy of three years who, for the last four months, has had this nasal twang and suffered from regurgitations of liquid food, after having gone through a slight diphtheritic affection of the pharynx, in the first half of January. The same remarks are applicable to other forms of paralysis, as strabismus, of which we have observed several cases in children. We may here state that we have seen no other forms than those two enumerated in infancy. In adults we have observed several instances of general paresis or either the motory or the sensory nerves, sometimes the two together. The lower extremities will be the first affected, and afterwards the sexual parts, upper extremities, hearing, taste, smell, and sight will be involved in the general affection. The motory nerves were mostly affected, physical exertion being extremely difficult. The youngest individual in which we have seen these general symptoms, was a lady of seventeen years, in whom motion, sensibility, sight, voice and intelligence suffered in an equal degree. The respiratory muscles have been reported, by authors, to have been paralyzed; we have not seen any such case, unless that reported on page 96 belonged to this class. As we had no experience whatever on this point at that time, we then directed no particular attention to this fact. The prognosis in these cases is favorable; all our paralytic patients recovered

in the course of from two to five months. Even complete amaurosis, of which we have not seen a case, is reported by Maingault to have recovered in a little more than six months. A few cases of death, however, have also been related; in two instances, from suffocation produced by food getting into the larynx, the pharyngeal muscles being paralyzed.

The cause of this paralysis is somewhat obscure. It is but natural that a material alteration taking place in the soft palate should interfere with its functions. But the majority of cases date from convalescence, will sometimes appear suddenly and sometimes are gradual in their development. It may be observed after very severe attacks of diphtheria, and again after apparently a mild form; we have stated that it followed some light cases of so-called herpetic angina; and we have above pointed to the fact, that many cases of diphtheritic paralysis were preceded by albuminuria. We do not hesitate to attribute general paralysis of this kind to the want of sufficient nervous power, produced by the diphtheritic hydræma (which, too, we consider in these cases as the cause of albuminuria). Local paralysis we are inclined, from physiological reasons, to attribute to local extravasation or exudation, the last cause of which must also be sought for in general hydræmia and facilitated transudation. We have seen one instance of loss of hair during convalescence.

The diagnosis of diphtheritic paralysis is made sure by the history of the case, and by its difference from other forms of paralysis. Paralysis from myelitis begins with clonic and tonic convulsions, and descends slowly, and the pharynx suffers last, whereas diphtheritic paralysis shows the affection of the pharynx and the power of speaking at first. Galvanic contractility is also intact (which is not the case in spinal paralysis); and paralysis, according to what has been observed in other epidemics, has a great tendency to localization in diphtheria.

As to the treatment of diphtheria and diphtheritic affections, we venture upon the following remarks:—This disease, which has been the subject of the foregoing exposition, has been shown to exhibit prominent symptoms of two different kinds, viz: local and general. Thus the treatment, where any is required, has to fulfill two distinct indications. We say, wherever it is required, for experience shows that a number of cases will get well without any treatment whatsoever, so that it is not a very unfrequent occurrence to meet with light tonsillar diphtherite without fever, or any other dangerous symptom, in children to which we are called for some other complaint. In such cases light fever may have been present, and large membranes have been exuded on or into the mucous membrane of the pharynx,

and nevertheless the whole course of the disease has passed unheeded and untreated. Such an occurrence is certainly not exceptional, for the same is true of other diseases, particularly those of zymotic origin. The majority of zymotic diseases require little or no medical treatment at all, especially those running their course in a distinctly typical manner. As to diphtheria, we have even made direct experiments, showing that mild cases will get well without treatment. But we think it more dangerous in diphtheria, than in other zymotic diseases, to abstain from treatment altogether, for three reasons:—Diphtheria is not a typical malady, but has a great tendency to return; it is more of an adynamic character than any other; and, finally, by the thick and extensive exudations in the pharynx and on the adjoining parts, it is apt to produce serious troubles, by mechanical encumbrances to deglutition and respiration.

The local treatment consists of cauterization of the membranes and surrounding parts with the solid nitrate of silver, or with strong or mild solutions of the same salt in water (℥ ss-j. : ʒj.); of gargles, consisting of solutions of (or applying in substance) astringents, such as tannic acid, alum, sulphate of zinc, or claret wine; in gargling with, or applying, such medicinal agents as are known to have some effect on the constitution and tissue of the pseudo-membranes, as chloride of potassium, chlorates of potassa and soda, diluted or concentrated nitric or muriatic acids, liquor of sesquichloride of iron, etc. Astringents will prevent maceration, render the exudation dry and hard, and alter the consistency of the surrounding hyperæmic and oedematous tissue. It will thus prevent, sometimes, the extension of pseudo-membranes to the neighborhood of the parts already affected, and in some cases may accelerate the expulsion of the membrane as a whole. We have thus seen the best effects from tannic acid, either applied directly to the parts by means of a curved whalebone probang, or dissolved in water as have a gargle (℥ss-ij. ; ʒj.) Of the tinct. sesquichlor. iron we seen no particular effect.

Cauterizations with nitrate of silver we have found to be generally of very little use when applied to the pharynx. Its effect is superficial only; it will form a scurf, but will destroy nothing. Destruction of the parts cannot be effected except by forcing the caustic into and below the membrane; this can seldom be done in the pharynx of children, and for this reason cauterization is unavailing at this point, but will prove beneficial, we believe, by confining the process of exudation to its original locality. In cutaneous diphtheria cauterization may be exercised to its full extent, but as these cases are generally

attended with extreme prostration, the general treatment will prove both more necessary and successful. If cauterization is to be resorted to, we generally use, and with good effect, more or less concentrated muriatic, or acetic, or nitro-muriatic acid. Where, however, cauterizations are made, great caution is necessary not to mistake afterwards the result of the caustic for pseudo-membrane. This remark is particularly applicable where nitrate of silver has been used.

In regard to the internal administration of remedies, and the general treatment of diphtheria, we have to remember that it is eminently an adynamic disease. Prostration will set in, and complete exhaustion will sometimes destroy patients in spite of the most careful treatment. As a general rule, therefore, no remedy should be administered that will increase the amount of water in the blood, accelerate its decomposition, or exhaust the nervous power. Mercury must be avoided, no blood be drawn, no vesicatories applied, in a word, no antiphlogistic treatment should be resorted to; for the superabundance of fibrine does certainly not indicate antiphlogistic treatment, as it is a well-known fact that in extreme hydræmia the proportion of fibrine may be greatly increased. The more hydræmia is increased, the greater the proportion of fibrine. For the same reason the careless routine practice of administering large doses of alkaline remedies, carbonates, bicarbonates, nitrates, etc., of potassa and soda, for the purpose of liquefying the fibrine, must be discarded. Their effect is generally not good, and we firmly believe that the duration of convalescence will be lengthened by their free use. Troussseau is certainly right in asserting that rationalism in medicine is very apt to lead to absurdities.

Emetics must be avoided as much as possible; but they will sometimes prove necessary to remove accumulations of mucus or macerated membrane. Then ipecac is preferable to the others. In cases where diphtheria is descending into the larynx, great caution is desirable in administering emetics; for in this form of croup exhaustion will sometimes take place unexpectedly and rapidly before suffocation.

The functions of all the organs have to be kept in order; the kidneys require special attention, the greatest danger being in the interruption of their function. Spir. nitr. dulc., squill, and parsley may be administered; but neither digitalis nor iodide of potassium. The skin will generally act well with liq. acetat. ammon., alcoholic beverages, and frictions. Regular baths, with alcoholic or aromatic admixtures, and change of air and residence will prove beneficial. Digestion must be kept as normal as circumstances will allow; tonic and stimulant diet not only allowed but insisted upon; meat, eggs, coffee, wine and brandy are recommendable.

As a general antidiphtheritic remedy, chlorate of potassa and chlorate of soda as its substitute, have earned a good reputation. Chlorate of potassa is soluble in sixteen parts of water, and is well tolerated by the stomach. There is no necessity, therefore, for preferring the soda, which is higher in price, though it has the preference in dissolving in three or four parts of water. We do not agree with those who deny the efficacy of this remedy, because it does not meet all emergencies. We have used it in hundreds of cases, besides its administration in stomatitis, mercurial affections of the mouth, etc., and are perfectly satisfied with the result, except in those cases which ran an extremely rapid course with such symptoms as high adynamic fever, quick and small pulse, evidences of dissolution of the blood. In such cases it is too slow in its effects. But in all those instances where a sudden and instantaneous effect is not required, and death is not imminent, it is a highly valuable remedy. But doses of a grain or two will not prove sufficient; it ought to be given in doses of from half a drachm to one and a half drachms daily, dissolved in water, alone, or combined with other remedies. If possible, the patient must be made to swallow the solution slowly, to have its local, as well as the general effect.

Acids are very beneficial agents in this disease. Diluted muriatic acid, four, six or ten drops every hour or two hours, more in proportion to adults, or concentrated nitro-muriatic acid, two or six drops in the same intervals, will be found to act as well locally as generally, and will, besides, increase the appetite and stimulate the digestive functions. The concentrated nitro-muriatic acid we have regarded as one principal remedy in those dangerous cases described above. Good effects have, in many instances, been observed from full doses of tannic acid, ten grains to two scruples and more being given daily, dissolved in water. It has a beautiful local effect in the pharynx when swallowed slowly, and is of invaluable service in renal affections.

As a tonic, iron is highly serviceable. The preparation most in use with the English profession, and with us also, is the tincture of the sesquichloride; its astringent effect has been praised highly, perhaps too highly; its influence on the whole system in general, and sanguification in particular, is undoubted. Its dose is, according to age, from twenty drops to one and a half or two drachms daily, alone, or, as most administered, with chlorate of potassa. Combinations of these, or the two with diluted muriatic acid, or each with the acid, are of the same practical value as they are theoretically justifiable. If tannic acid and iron were to be used, in any form or man-

ner, at the same time, they are to be kept apart, and administered at different times; the tannate of iron being indigestible. Ozanam's formula, bromine *gt. j.*, bromide potass. *gr. ij.*, aq. $\mathfrak{Zij.}$, we have tried in fifty cases, from November, 1859, to February, 1860. We have used it in much larger doses than Dr. O., from thirty to fifty drops and more daily, after we were dissatisfied with smaller ones. We think it a valuable remedy except in severe cases; we should not then rely on it; perhaps, however, we have not sufficient experience. We have a similar remark to make in regard to the tincture of iodine, with which we have had but little experience.

Of the greatest value is that powerful febrifuge, quinine. We have but seldom used it as a tonic, but generally in one or two large doses (from five to ten grains in children, or the sulphate of muriate, the latter containing more quinine) daily. We have never seen any bad effects, but have always found a great and rapid remission of the fever. If one dose was taken, we ordered it in the afternoon, usually between three and five o'clock; another was sometimes taken in the morning for one or two days, until there was no necessity of administering it twice. A *conditio sine qua non* is a full dose. A child of a year or two must not have less than five grains in a daily dose; we have even given to children of two or three years, repeated doses of ten grains, and have been fully satisfied with its effect. We have in no disease observed less cerebral symptoms attributable to the effect of quinine than in diphtheria. It will prove particularly successful in such cases as are more or less complicated with acute rheumatism. In the case of a girl of seven years, whose stomach was much disordered, we resorted to subcutaneous injections of a nearly neutral solution of mur. chin. *gr. iv.*, on two subsequent days, with excellent success.

Albuminuria requires tannic acid; we seldom give any other remedy, and warmly recommend it. The functions of the bowels, skin, etc., require the care indicated by the rules of general and special pathology. In a case of hemorrhage from and suppuration in the kidney, in a sickly diphtheritic girl of three years, we thought proper to give veratrum, in order to diminish the fever, and thus, indirectly, to relieve the congestion of the kidneys; our success was complete; the child being more healthy and robust than for years.

Descending croup, during the epidemic, proved highly dangerous. Patients in some cases had not even sufficient time to die of suffocation; but perished from the exhaustion brought on by the general malady. We feel, therefore, justified in warning against a free use of emetics, as a new source of exhaustion. We have no particular remedy to recommend; the

treatment should vary according to the case. But, in referring to our remarks and cases above, we feel sure that even of such cases many will in future be saved by tracheotomy.

Hemorrhages require, locally, astringents, and the general antidiphtheritic nutrition and treatment. Wherever the seat of hemorrhage can be reached, we doubt not but the application of tannic acid, muriated tincture of iron, or, better than anything else, persulphate of iron will prove successful instantaneously.

Paralysis requires moderate, very moderate, active and passive motion. The eyes require great care and perfect quiet. Local paralysis indicates local galvanization; thus, in paralysis of the soft palate, one pole is applied to the palate, and the other to the mastoid process. In general paralysis, iron and strychnia are indicated; the latter we have used, on the recommendation of Trousseau, until a slight convulsive flexion, usually in the thighs first, became perceptible. Our dose was, in adults (as we have not seen children with general paralysis), from the one-eighth to the one-sixteenth of a grain, twice a day.

Submaxillary and cervical adenitis require seldom or never any kind of depletion. In two cases only, in which active inflammation seemed to take place, we have applied leeches. Our usual treatment consisted in the application of camphorated oil, or of tincture of iodine in older cases, or of the following formula: Iodine, $\mathfrak{z}\text{ij}$.; glycerine, volatile liniment, aa $\mathfrak{z}\text{ss}$. In the very small number of cases in which suppuration took place we recommend early incision.

Finally, in cerebral affections, we know of nothing to recommend. Convulsions in the onset of the disease will generally not prove fatal; but such as occur after exhaustion and symptoms of dissolution of the blood have taken place, will prove the prelude to death.

CHRONICLE OF MEDICAL SCIENCE.

MATERIA MEDICA AND PHARMACY.

1. *The Whey and Grape-Cures in Germany and Switzerland in the Treatment of Chronic Diseases.*

In France, with some few exceptions, no attention has been methodically devoted to the whey and grape cures; they are, in general, not considered sufficiently important to deserve much consideration. In Germany and Switzerland the reverse is the case. Whey and grapes are there not only popular as a means of treatment of disease, but also have a place assigned in the important class of mineral waters, with which they are associated under the name of organic mineral productions, and an increasing number of patients flock every year to the various establishments devoted to their exhibition.

Dr. Lersch, one of the German authors who have best treated this subject, estimates at three hundred the number of these establishments, a figure which has since been nearly doubled. In the North, Rehburg, in Hanover, where goat's whey is distributed, is one of the most important. Liebstein, in the principality of Saxe-Meiningen, Rosenau, in Moravia, also deserve special mention. Schlangenbad, in the duchy of Nassau, is one of the most agreeable places among those devoted in the region of the North to the sero-lacteal treatment. In the Southern regions, the most frequented establishments are those of Baden-Baden, Badenweiler, Gleisweiler near Landau, Beuron, Ischl, the most celebrated spa of the Austrian empire. The whey taken there is principally that of the ewe, preferably prescribed for pulmonary phthisis. In Styria, there exist many whey establishments, which are, at the same time, important as spas, for instance, Neuhaus, Rohisch, etc. But the Southern station of all the most deserving favor is in the Alps, and within sight of Italy, Méran, a locality which, for its climate, its whey, and its grapes, is the most renowned in Germany; its fame has even reached the interior of Russia, and, thanks to its average temperature of $36\frac{1}{2}$ degrees (Fah-

renheit) in January and February, it is peopled in winter with numerous invalids. But whey exists wherever flocks are to be found, and for this reason it is the object of such extensive speculation in Switzerland. Most usually, this kind of treatment is instituted in thermal establishments, or in their vicinity, because it has been ascertained that whey mixed with mineral water, and exhibited either in beverage or more rarely in baths, imparts new virtues to both these medications, sometimes increasing the activity of the whey, and at others tempering the too great power of the springs. Mr. Carrière observes, with regard to phthisis, that the mineral waters, in which whey proves most beneficial are the sulphureous, which contain much chloride of sodium. The author describes as follows the manner in which the sero-lacteal treatment is practiced in the principal establishments of Germany and Switzerland:

“In order to measure with precision the quantity of whey exhibited, glasses are used similar to those of Carlsbad, which contain about 4 oz. of liquid. The first dose is taken fasting, and the second after a quarter of an hour’s walking exercise in the open air or under shelter, according to the weather. It is almost indispensable that the whey should have been recently prepared, although, as we have already stated, excellent precautions are taken to preserve its temperature. The establishments, considered the best, renew their stock three times a day in order to insure its freshness. It is an advantage, not without value, to take the whey on the very spot in which it is prepared, or very near it. If it should come from afar, it is better to drink it at the springs, like a glass of mineral water, than to wait for its distribution. In the early stages of the cure, two glasses are not exceeded; if no obstacle should arise, and no great perturbation of the digestive organs occur, the daily dose may be increased to four or five glasses, equivalent to about $1\frac{1}{2}$ pints of whey. This applies to cow’s milk whey only, according to Dr. Mojsisovicz, from whom we borrow all these details. But for goat’s or ewe’s milk-whey, both less digestible, and applicable, especially the latter, to the cure of pulmonary phthisis, it is essential to proceed with greater moderation. Patients laboring under tuberculosis should never take more than three glasses, at intervals of at least half an hour. Two glasses should be drunk in the morning fasting, the third towards the middle of the day. It is not, however, possible to lay down absolute limits on this point. In consumptive cases especially, it would be difficult to establish beforehand how the cure is to be continued after the first days. The practitioner must, in the first place, study his patient, and then act according to the symptoms and complica-

tions which may arise ; but he must especially not allow himself to be discouraged. Whey is not one of those remedies, the efficacy of which proceeds by rapid and unexpected changes ; its action is slow, and patience is necessary. Perseverance has, in this treatment, been the secret of many cures.

“Invalids should not confine themselves to a short season devoted to the treatment, says Dr. Helfft ; the cure lasts from six to eight weeks at most, which is but a short time for a result of any importance. Not only is it desirable to resume the course of treatment, if possible, after an interval of rest, but it is still better to continue it at the new residence. If the patient is consumptive, and takes up his winter quarters in a mild climate, an additional reason exists to strengthen the influence of the climate by that of the remedy. It is thus the most favorable chances are combined for the attainment of the final result.”

The diseases in which whey would seem to be productive of most benefit, independently of all theoretical interpretation, are chronic bronchitis and incipient phthisis, obstruction of the viscera occasioned by intermittent fever, the abdominal form of hypochondriasis, hemorrhoids, obesity, hyposthenic affections in women and children, nervous derangement kept up by debility, etc.

“The grape-cure,” says Mr. Carrière, “consists in making entire meals several times a day exclusively of grapes. These repasts, added to others, supply for the day an amount of nutriment sufficient to satisfy the best appetites. Patients begin with a pound, and progressively increase the quantity to two, three and even six and eight, the extreme limit usually attained ; few consume larger quantities.

“The first portion must be taken early in the morning, not at home, but in the vineyard, when the sun has not yet absorbed the humidity on the grape and the fruit is in all its freshness. This recommendation does not apply to consumptive patients, for whom the early morning influences are unfavorable and even dangerous. The sun must have heated the lower layers of the atmosphere, for the advantages of exercise not to be annihilated by an aggravation of the symptoms. The early repast in the vineyard, in the first haze of morning, when the temperature is still low and the wind cool, is suited for such organizations or idiosyncrasies only as require motion in the free oxygenized air to urge the circulation, and rouse the system from its inertia. The first meal should be the most copious. The stomach is empty and can receive more food than in the course of the day. The other grape repasts must be regulated so that the doses of fruit may be nearly equal.

The morning walk should last two hours, when a breakfast consisting of bread and water should be taken. If the weather is unfavorable for walking exercise out of doors, elegant rooms for the purpose are to be found in all such establishments, sheltered from the inclemency of the atmosphere, which is generally not to be depended on in mountainous countries. The second grape meal should precede dinner, which takes place about two o'clock; the third at four or five; and the last, a few minutes before bed-time, and almost immediately after the light repast which closes the day. This system is persevered in regularly for five or six weeks, not until the cold drives patients away from the establishments, but until the vintage has completely stripped the vine-stocks.

“Some monographers carry their recommendations too far, and advise the invalids to avoid swallowing the skins and stones, because both are difficult of digestion. The treatment should not be rendered troublesome by unnecessary precautions. The grape-cure is one of those in which the greatest liberty should be left to the patient, not with regard to the regimen properly so termed, but to the treatment. If he can bear well the few pounds of grapes he takes in the day, he may increase the dose, or even exceed the prescribed limits. This kind of imprudence will generally present fewer inconveniences than advantages, and will give seldom rise to regret.”

The whey and grape-cures, particularly the latter, were well known to Professor Chomel. In his *Treatise on Dyspepsia*, this eminent physican recommends them under the denomination of extra medical treatment, as suited to impress the mind favorably, and subsequently to re-act with advantage on the system.

In Mr. Carrière's estimation, the predominant virtue of the grape is observable in diarrhœic discharges, even in their gravest forms. The various diseases which derange the functions and affect the nervous system of the digestive organs, may likewise be remedied by this treatment. The grape-cure is also efficacious in abdominal and hepatic plethora and their attendant affections or complications, such as obstruction of the spleen, of the larger vessels and hemorrhoids. It is not less beneficial in the principal varieties of discrasy, such as scrofula, tuberculosis, and pulmonary phthisis, gout and cutaneous affections. Finally, it advantageously removes hyposthenia and its concomitant symptoms, whether proceeding from a peculiar condition of the constitution or from causes of a different order.

2. *The Physiological Action of Chlorate of Potassa.* (Translated for the Boston Medical and Surgical Journal, from a Memoir read by M. E. A. ISAMBERT before the Société de Biologie, 1856.)

Dr. Isambert experimented upon himself and others with the following results. He found, 1st, that the chlorate of potash is absorbed and eliminated with great rapidity, and that it is eliminated unchanged, (still in the form of a chlorate,) and consequently cannot furnish oxygen to the system, as has been supposed. 2d. That the principal channels for its elimination are the salivary glands and kidneys. Traces of it are detected in the saliva within five minutes of its ingestion, and ten minutes later in the urine. At the end of an half hour the process is at its maximum intensity, and it persists from fifteen to thirty-six hours. The duration of the elimination does not seem to depend upon the dose. It was nearly the same, with doses ranging from one to twenty grammes daily. Dr. Isambert has detected it in the milk of two nurses who were taking it as a medicine.

He has found it in the nasal mucus, the tears and perspiration. Its presence in the fæcal matters he regards as doubtful, but thinks there is reason for believing that it passes into the bile.

3d. Dr. I. has ascertained the following facts in regard to its physiological action :

Doses from one to four grammes daily produced no appreciable results.

Doses of eight or more grammes induced a decided impression, lasting two or three hours ; this was accompanied by a saline taste, apparently identical with that of the chlorate of potash. Although less powerful than a mercurial salivation, the results of over-stimulation were apparent in a weakness of action of the salivary glands for five or six succeeding days. A slight alteration in the voice was noticed. The intensity of the salivary excitation was proportional to the dose administered.

The only constant effect upon the digestive function was an increase of the appetite. In one case, a slight pyrosis followed the use of a large dose, given in too concentrated solution. It never occasioned diarrhœa, although the fæces were often tinged with green.

In high doses it possessed well-marked diuretic properties. Twenty grammes daily induced frequent micturition, and a slight sense of pain and weight in the lumbar region. The

urine, during the whole time of its elimination, was *strongly acid*, and deposited urates abundantly.

It slightly increased the secretion of the pituitary membrane.

Its action upon the respiration, nervous system and circulation (the latter being in a healthy state) was inappreciable. When the circulation was unduly excited, it seemed to exert a sedative influence upon it.

No inconvenient effects have been experienced from taking large doses for several consecutive days. M. Socquet has taken thirty grammes (gr. 450) with impunity.

The facts above stated show that it acts in a manner similar to the iodide of potassium, and has little resemblance in its properties to the alkaline carbonates, among which, as a therapeutic agent, it has been classed.

Dr. Isambert devotes a large portion of his paper to the discussion of the uses to be made of the chlorate of potash in therapeutics. The results are precisely what one would infer from a careful study of its physiological action.

3. *Food for Babes, or Artificial Human Milk, and the Manner of Preparing it and Administering it to Young Children.* By Dr. CUMMING, of Mass.

Dr. Cumming proposes a plan for supplying artificial human milk to infants unable to procure from the maternal breasts nutriment of the proper quality and quantity. He has adopted this plan in his own household for a number of years with the most gratifying success. He claims that it produces a wonderful immunity from colic, pain in teething, and various disorders of the stomach and bowels; that it contributes materially to uniform growth, prosperity, vigor and health, if it does not secure it; and that, in many cases, it will relieve almost instantly the distressing symptoms of wasting diarrhœa, &c. He states that, in composition, it closely resembles the natural secretion of healthy and vigorous mothers, and contains all the ingredients necessary for the proper growth and development of the child. This artificial human milk is to be obtained in two ways:

1st. By taking the *upper third* of cows' milk that has *stood* for four or five hours; this containing 50 per cent. more butter than the ordinary milk of the cow.

The second, and in warm weather, the better way, is to take the milk from the latter half of that given by the cow, (containing the "strippings,") taking care that the cow be milked dry. In both instances, the milk is to be diluted with $1\frac{1}{2}$ parts

of soft water, and properly sweetened with loaf sugar. The animal from which the milk is to be taken, must be from four to ten years of age, and free from disease of any kind, it being unimportant that she should give a large quantity of milk. Her calf should not be less than two weeks old, and when it becomes four or five months old, the cow is to be given up and another selected. The best feed for the cow is hay and salt and water, which will improve the quality of the milk though the quantity may be less than when other articles of food are employed.

Various dilutions are of course required for various ages.

Thus for the first two weeks after the child's birth it is to be furnished with an *artificial colostrum* which requires the use of the upper *eighth* instead of the upper third of milk which has stood for four or five hours; or, the employment of the last *tenth* of the milk furnished by the cow.

A schedule is given, arranged to suit the wants of vigorous children of various ages.

Attention is to be paid to the physical condition of the child as well as its age, in preparing the required dilution.

The milk should be prepared twice a day in warm weather, unless kept on ice.

The milk is to be administered by means of a bottle, with the neck occupied by an artificial nipple composed of a goose quill rolled up in a strip of muslin; all of which are to be kept scrupulously clean.

The milk should be given at regular intervals, the child taking at each time as much as it wants; and the child should be trained to pass six or eight hours at night without being fed.

The temperature of the milk when given should be about 100°; it should be taken slowly, and the flow from the bottle controlled by a proper arrangement of the quill and muslin.

Dr. Cummings thinks this mode of feeding should be continued until the children obtain their full set of teeth, or to nearly the age of two years—or at least rely exclusively on it until sixteen teeth are fairly developed, when other food may be gradually commenced with.—*Medical Reporter*.

4. Use of *Asarum* in Alcoholism. By DR. SMIRNOFF.

Dr. Smirnoff states that he has become convinced by repeated trials that the *asarum Europæum* well deserves the reputation it has obtained in Russia of being an excellent remedy for the effects of drinking. The influence of a continuous abuse of alcoholic drinks is first exerted locally, but afterwards dys-

pepsia is produced ; and the nutrition and functions of the entire economy, especially of the central portions of the nervous system, becoming interfered with, the blood itself being loaded with an injurious foreign material, the *dyscrasia potatorum* is at last completely established. The *asarum* fulfills various indications, acting beneficially on the alimentary canal in those cases in which the digestive powers are so much at fault. Its aromatic principle confers upon it a stomachic power, and regulates the condition of the intestinal discharges, producing vomiting and purging when given in large doses. Its most beneficial action, however, is manifested on the defective appetite, and by its counteracting the invincible longing for alcohol. The horrible sensations with which the drinker awakes in the morning, and which impel him to seek temporary and delusive relief from renewed libations, are much blunted and mitigated by means of a glass of strong infusion of *asarum* and some other nervine—*e. g.* *valerian*. Its immediate effect is often to produce vomiting, and sometimes purging ; but the painful sensations at the epigastrium undergo relief, and the appetite becomes invigorated. Persons who have been long habituated to alcoholic drinks cannot, however, have these suddenly suppressed with impunity ; and in such cases the author gives the *asarum* in brandy, applying at the same time a blister or an issue to the pit of the stomach. By this means the normal activity of the stomach becomes excited and the longing for alcohol diminished. The author, however, cannot agree with those who would still allow a small quantity of spirits to habitual drinkers, even when the morbid desire for it has become appeased. The continuous use of a decoction of *asarum*, even when it does not succeed in extinguishing the desire for alcohol, always supports the powers of the patient ; and it is remarkable in some cases, in which the individuals have been long accustomed to periodical intervals of drunkenness, ending in delirium tremens, how much longer these intervals will become, and how much less likely delirium tremens is to recur. The patients themselves are sometimes surprised at the comparative impunity with which they can continue their drinking. The author prescribes three or four glasses a-day of an infusion made with \mathfrak{z} ij. of *asarum* root, \mathfrak{z} j. of *valerian* root, and $\mathfrak{z}\frac{1}{2}$ of orange-peel, but he does not state the quantity of water employed. In cases of drunkenness another formula is composed of decoction of *asarum* (made by boiling from $\mathfrak{z}\frac{1}{2}$ to \mathfrak{z} j. of the root) \mathfrak{z} vj., tinct. of *valerian* \mathfrak{z} ij. to \mathfrak{z} ijj., Sydenham's laudanum gtt. xij., syrup of orangepeel $\mathfrak{z}\frac{1}{2}$. A tablespoonful of this is taken every two hours. He finds from two to five grains of bismuth taken four times a-day a valuable adjunct. He has

also found the following popular Russian remedy of service in cases of drunkenness:—R. Ammon. carb., $\mathfrak{z}\frac{1}{2}$; aceti vini, ℥j.; oxymel scill., $\mathfrak{z}\frac{1}{2}$. Two tablespoonfuls every two hour.—*Med. Zeit. Russland*, 1859, No. 8.

MEDICAL PATHOLOGY AND THERAPEUTICS.

1. *Discussion on Diphtheria.* New York Medical and Surgical Society. (Reported in the Medical Times.)

Experience of DR. CLARK.—Dr. Clark stated that, on the evening after the last meeting, he was called in consultation by Dr. Crane to visit a family in Elizabeth, N. J. Six, out of eight, children were suffering at the time from scarlet fever, and one was lying dead in the house. Three out of the six children presented diphtheritic membrane in the fauces, and the remaining three had swollen tonsils with more or less inflammation of the throat. One of them had some white spots upon the inner surface of one of the tonsils, which at first looked a little like membrane, but afterwards turned out to be nothing more than a white secretion in the follicles. Two of them were at that time, as we supposed, desperately sick, and in one of these the membrane was distinctly discoverable in the nasal passages. The voice was a mere cry. The breathing was not as much obstructed as in croup, but sounded as if a valvular structure was playing up and down over the opening of the larynx; and we took it for granted that if the membrane had not already, it would eventually extend into that portion of the breathing apparatus. The pulse 140, and the intelligence nearly abolished. The patient was lying with her eyes closed, paying no attention to anything that was said, and considerable force had to be used to open the mouth. She moaned with almost every breath, though occasionally she would get a little quiet and seem to be asleep. This child finally recovered.

In one of the other children, the nasal passages were entirely plugged up by the drying of the secretions that flowed down from the external opening. The constitutional symptoms with him too were very marked. His pulse was the same as the others, but, instead of being semi-comatose, he was restless,

dozing continually. He lived nearly a week from the time I refer to, and apparently died from exhaustion, the result of the occurrence of numerous ulcerations very much after the manner of bed sores. It struck him that this latter feature of the disease was an evidence of the constitutional influence of the poison. The father, who was fifty-seven years of age, also had the diphtheritic exudation in the fauces, but in him none of the symptoms of scarlet fever had presented themselves. He, however, had the same character of valvular breathing as noticed in the daughter. His tonsils and velum were very much swollen, and the glands on the outside of the neck moderately so. The moment he lost consciousness in sleep, his breathing would stop as if something had passed into the opening of the larynx and prevented the entrance of air. The inspiration alone was obstructed. His friends were unwilling to allow him to sleep at such times for fear he would suffocate. This difficulty of breathing did not seem to me to be dependent upon the existence of a membrane, but upon the swollen condition of the hanging portion of the fauces, which dropped fairly down upon the top of the larynx. As soon as the inflammation subsided, this symptom passed off. At the time we saw him, he had been in a state of active delirium for forty hours; his pulse was about 100 per minute. He finally recovered. The treatment for all these cases was about the same; pretty active stimulation with alcohol and the very free use of the sulphate of quinine, and the local application of nitrate of silver in solution. There was a circumstance that interested me in connection with the two children who had the membrane in its worst form, relative to scarlet fever. In the girl, the eruption was out full for eight days, and when we saw her was perhaps subsiding a little; in the boy, the symptoms had been out eleven days, and was still vivid. Desquamation was quite active, and the scales were standing out, attached to the surface by their edges, in all possible directions; rubbing these off, the eruption could be seen as on the second or third day. The urine in these cases was not examined.

Experience of DR. METCALFE.—Since the beginning of the winter I have had ten cases of this disease, six of which I have seen in consultation. There have been seven cases in which the diphtheritic deposit affected the throat mainly, in the others the Schneiderian membrane was the principal seat of the exudation. The first case was a child three years of age, who was dying when I saw it; both tonsils and part of the velum were covered with the membrane. The patient died comatose. The next was a sister of this child, who presented the exudation on each tonsil, the palate, and in the nostrils;

there was a good deal of constitutional excitement, with occasional delirium, present. This case terminated favorably after a fortnight's illness. The third case was a brother of the last, eight months old; the membrane was situated on the surface of the tonsils, and invaded a small extent of the palate. This child recovered after four weeks illness. The uncle, who was in the house, convalescent from measles, had a slight diphtheritic patch on the palate. The mother also had some trouble about the throat, her tonsils were much reddened, and the peculiar coating could be scraped from their surfaces without much difficulty. The constitutional disturbances were very trifling, and in two or three days she was entirely recovered. The next was a little girl four years old; I saw her on the next morning after the night she was attacked, when I found both tonsils almost completely covered with the membrane. The pulse ranged from 160 to 180. The breath was horribly fetid. The exudation in the course of the next day spread so as to cover the palate, and the grave symptoms increasing, the child died of apnoea two days after. The next was a child twelve years old, of a delicate constitution, who was taken on a Sunday morning, the membrane covering both tonsils and the edge of the soft palate. On Monday, he was somewhat better; on Tuesday, the fever subsided, and the membrane disappeared. That night the membrane re-appeared, and extended into the nostrils; together with this there was attendant an immense tumefaction of one side of the neck. In consequence of this, there was a good deal of constitutional excitement, delirium and difficulty of deglutition. The child, after making us believe for the greater part of four days that she was going to die, finally became convalescent. In this connection, Dr. M. exhibited a beautiful cast of membrane which had separated itself from the tonsils. Another case was in a young man, a member of the class at the University. He was taken sick on Saturday, and showed the patches in his throat the day following, when he experienced some difficulty in deglutition; had fever, debility and quickness of the pulse. These symptoms continued for three days; he suffering a great deal without being, as I thought, in positive danger. On the fifth day after the commencement of the attack, he was suddenly taken with a rigor, his skin was cold and covered with perspiration—respiration forty per minute. He could not lie down for a minute without having symptoms of suffocation. The gentleman who saw him with me was of the opinion that the case would terminate fatally very soon; the patient, however, recovered, and was able to return home on the Monday following. These are the only cases worthy of mention; of the rest,

with but one exception, there was very little constitutional excitement—some quickness of the pulse, some pallor of the body, restlessness, pain in swallowing, and the occurrence of a well-marked membrane, with nasal defluction—and they all got well. I have not used quinine in any of the cases, but in its stead the mur. tinct. ferri in twenty-drop doses every two hours to adults, decreasing the quantity according to the age of the patient. Besides this, I give plenty of beef-tea, milk-punch, and wine whey. I have used the sol. of nit. silver locally, but can't say that I have derived any benefit from it. I have given the chlorate of potash as a gargle, but there again I failed in obtaining any good results. In conclusion, Dr. Metcalfe referred to a new remedy, the iod. of bromine, which had been brought to his notice by a physician in Long Island. It was used locally in the strength of fifteen drops to eight ounces of syrup, and was of great service of correcting the fetor of the breath. He (Dr. M.) had succeeded very well with the remedy, and advised the members to give it a trial.

2. *Discussion on Diphtheria.*—Proceedings of the St. Louis Medical Society.

Experience of DR. STEVENS.—If diphtheritis is a blood disease, may we not conclude that the blood is consequently more disposed to retain effete matters, and less inclined to take in oxygen. This formidable disease should at this time engage our serious attention, assuming as it has for several months the character of an epidemic, attacking persons of all ages and conditions. The insidiousness of its attack, and the obstinacy with which it resists ordinary antiphlogistic treatment are known by us all. I hope all members who have had opportunities of observation in the diagnostic signs, nature, and treatment of this malady, will communicate such information, so that we may be prepared to do our duty fully to those who come under our care.

Experience of DR. M. M. PALLER.—I cannot regard diphtheria as including croup, if we suppose croup to be the disease which was so graphically described by Dr. Francis Howe in 1765, and which has been acknowledged, from that period to the present time, to be an acute inflammation of the air passages, distinguished from others by its rapid progress, by the existence of a concrete exudation in the larynx, and by its attacking principally children from the age of one year up to seven or eight years. I believe that diphtheria is a constitutional disease, and its local manifestation is a sore-throat,

attended by a pellicular exudation. Sometimes the constitutional symptoms are so slight, that the disease really requires no treatment whatever. In others, the most marked depression of the vital powers commences at once. Between these extreme degrees, there are many varieties; and, consequently, there can be no predetermined method of treatment. Each case has to be treated upon its own indications. As a general rule, however, depletion is not required—a mild purgative may be necessary at first, to be followed speedily with stimulants, and nourishing diet. I do not attach a great deal of value to local treatment, but I do not discard it. Dr. West has great confidence in a strong solution of nitrate of silver—I prefer the chlorine solution. I place considerable reliance in the administration of chlorate of potassa. I cannot agree with Dr. Stickel in the propriety of the application of a blister to the throat—I fear that such a procedure would lead to sloughing. Some curious phenomena follow diphtheria—bullæ make their appearance in the groins and on other parts. These often become the seat of cutaneous exudation; but they do not form a brownish thick crust, but as the bullæ become flaccid from the absorption of its contents, a firm concretion can be distinguished beneath the epidermis; this is found, on the removal of the covering, to exhibit all the characters of the diphtheritic exudation. Sometimes it happens, after diphtheria is cured, pains attack the joints; the limbs lose their strength, and the patient's movements are staggering and uncertain; the muscles concerned in swallowing and in the voice seem to be partially paralyzed, and his deglutition is seriously interfered with, and his voice reduced to a mere whine or whisper. Most frequently they recover from all these, under the proper treatment, which consists in giving tonics and stimulants. In other cases, they become more and more feeble and die. The appearance of albuminuria in diphtheria has led some to suppose that the disease is a modification of scarlet fever. I cannot agree to this—I think there is a specific difference.

Experience of Dr. MCPHEETERS.—I believe that in diphtheria, as in most other severe forms of epidemic disease, the results of treatment depend far more on the character of the case than on the kind of treatment instituted. The mild cases will recover under different and even opposite modes of treatment, while the severe ones will die in spite of all that the physician can do. Still, I believe that much may be accomplished by proper medication, and that there is a right and wrong method of treating this and all other diseases.

The almost universal tendency to sore-throat which has pervaded this entire community for several months past, I believe

to be owing to the diphtheritic tendency, or, in other words, that it is a modified form of diphtheria, in some instances attended by a deposit on one or both of the tonsils, which, when removed, leaves a ragged, strawberry-like ulcer. These throat affections, generally, yield readily to the expectant plan of treatment, and in many instances recover without treatment. The fatal cases of diphtheria which I have seen, have all been of the croupy form of the disease, in which hoarseness was a prominent symptom from the beginning, and in which there was a rapid exudation of the peculiar diphtheritic formation, extending speedily into and blocking up the air passages, and causing death from apnœa. In such cases, I am satisfied that emetics are injurious from the prostration they give rise to; I would, therefore, rely on tonics, stimulants, and a general sustaining plan of treatment. Carbonate of ammonia, wine, quinine, chlorate of potash, and in some instances oil of turpentine. The great principle, however, to be borne in mind is, that as diphtheria assumes different phases, the treatment in every case must be modified so as to meet the varying indications.

3. *On Diphtheritic Paralysis.* By Professor TROUSSEAU.

Is diphtheritic paralysis a new disease? Those of my hearers who commenced their studies some years ago, must have become struck with the sudden predominance of new pathological conditions. During the last thirty years we have seen several of these so-called new diseases, such as Bell's disease, or facial paralysis; Bright's disease, or albuminuria; leucocythemia, or Virchow and Bennett's disease; endocarditis, or Bouilland's disease; affections of the suprarenal capsules, or Addison's disease, etc., etc. Since the period when Calmeil published his admirable work on epilepsy, how common has this terrible neurosis become, while forty or fifty years ago it was comparatively a rare affection. Thirty years since the cholera was unknown in English and French India. The plague still so common in certain regions, tends to completely disappear in others. Variola, pertussis, and scarlatina, which at a very remote epoch prevailed in great intensity, and later had almost disappeared, have been in their turns as new diseases. M. Broca, while *interne* to Blandin at the Hôtel-Dieu, never failed, when examining the bodies of those who died of purulent infection, to detect suppuration of the veins. He states, however, that at the present time he finds such phlebitis quite an exceptional occurrence. At the beginning of 1859, M. Delpech observed an epidemic of puerperal fever at the Maternité, in

which suppurative fever was the marked characteristic, pus being deposited in the veins and as metastatic abscesses, while there was scarcely any pain in the hypogastrium. In another epidemic, at the end of the year, on the contrary, it was puerperal peritonitis which carried off the patients. In Medicine as well as in Surgery there are peculiar physiognomies of diseases; one revolution, the intimate essence of which escapes us, brings them on, another dissipates them, and a third re-establishes them.

As to diphtheritic paralysis it is probable that it has never been so prevalent as within the last two or three years. M. Trousseau has performed the operation of tracheotomy successfully on 60 children (out of 250 operations,) and in only 2 of this number have general phenomena, analogous to those in question, been observed. Diphtheria itself, indeed, has of late years taken on an altered physiognomy, for in place of invading the pharynx, and proceeding slowly towards the larynx, it now becomes generalized with a terrible facility, prostrating the patients, and delivering them up to the mercy of nervous accidents of the highest intensity. "I never witnessed such cases with Bretonneau, although my master subsequently signalized these manifestations of a novel malignity."

In answer to the question whether a simple angina may not sometimes lead to a paralysis, M. Trousseau adduces some cases in which paralysis of the velum seems to have followed it; but when the paralysis has become more generalized, he suspects that it may be due to diphtheritic influence, although this may not have been accompanied by its usual local manifestations—just as is the case with instances of scarlatina, accompanied by little or no eruption.

In 1771 Samuel Bard described a case of aphonia following diphtheria, and Bretonneau reporting this fact in 1826 stated that he then had met with nothing analogous to it; but in 1843 he witnessed the production of a paralysis of the extremities following diphtheria, in the person of M. Herpin, a Surgeon of Tours, who had contracted the disease in consequence of some of the false membranes from the throat of a patient which he was syringing gaining access to his nostril. Since that time, M. Trousseau has paid much attention to this complication; but believing at first that the paralysis of the velum might be due to muscular inflammation, it was not until 1853 or 1854 that he definitely drew the attention of Practitioners to the fact that the paralysis really depends upon a more general condition, and that it is in fact one of the effects of the poisoning of the entire economy by the diphtheria. M. Maingault has especially of late done most towards the elucidation of this

disease, which has not only multiplied itself at the Children's and St. Eugenie Hospitals, but has become so common in private practice that perhaps there is not a single Practitioner in Paris who has not seen one or more cases.

Although in general the very varied paralytic symptoms in diphtheria may cease, even without the intervention of medicine at the end of two, three, four, or six months, there are other cases in which death may be the result, or the paraplegia may become more enduring. Examples of this are adduced by M. Trousseau from his own practice. In some cases death has resulted from the mere difficulty in swallowing.

As to the nature of this paralysis, it is evident that it is not dependent upon a material lesion of the brain, as this would be inconsistent with the versatility of symptoms observed, and with its frequent curability. It is presumable that there is something analogous to what is observed in certain cachexias. In Bright's disease paralytic phenomena are also observed, and the amaurosis in that affection has been well made out and described by M. Laudonzy and others. One very remarkable circumstance in diphtheritic paralysis, is the temporary extinction of venereal desires, which occurs at a very early period, even in those possessed of considerable genital ardor. In various other serious pathological conditions, especially phthisis, the patients long preserve their copulative aptitude. M. Trousseau referred to other instances of paralysis, analogous to those now in question, occasionally observed after feeding on certain poisonous fish, after typhoid or variola, after asphyxia from charcoal vapors, and after the manipulation of sulphuret of carbon in the vulcanized caoutchouc factories (where also both men and women experience venereal frigidity.)

In the treatment of paralysis consecutive to diphtheria, while combatting the local condition, we must especially endeavor to restore the patient's strength. Bark in all its forms, iron (especially the syrup of the ammoniacal citrate or the perchloride,) bitters, animalized and nutritious diet, exposure to the fresh air, dry frictions along the the spine, aromatic and stimulant lotions, and sulphureous baths, comprise our most precious resources. Preparations of nux vomica, and especially the syrup of the sulphate of strychnia, act as general stimuli, arousing the muscular contractility, and may, on occasions, render good service. In spite of these and all other means, it must always be borne in mind that diphtheritic paralysis is an affection of considerable duration.—*Gazette des Hôp.*, 1860, Nos. 1 and 5.

MISCELLANY.

ARAGO ON FRENCH PHYSICIANS.—Preference to the most suffering, to those in most danger, disregarding rank and fortune, such was the sublime rule of the medical corps ; and such is still its gospel. I want no other proof than the words of Larrey to his friend Tanchou, wounded at Montmirail : “ Your wound is slight, sir ; in this ambulance we have room and straw for serious wounds only. They will take you into that stable.” The sentiments of the medical body towards the suffering poor are not to be doubted in the country where Antoine Petit said to the irritated Marie Antoinette : “ Madame, if I came not yesterday to Versailles, it was because I was attending the lying-in of a peasant, who was in the utmost danger. Your majesty errs, however, in supposing that I neglect the dauphin for the poor. I have hitherto treated the young child with as much attention and care as if he had been the son of one of your grooms.”—*Arago's Biographies*, 1st series, p. 163, Eng. Trans.

BOSTON MUNIFICENCE.—The late Jonathan Phillips, whose name was honorably associated during his lifetime with various institutions for the promotion of science and exercise of benevolence, has bequeathed a large portion of his property to these objects. Besides large legacies to the three colleges of Massachusetts and to several public libraries, he gives ten thousand dollars to the Massachusetts Medical Society, an equal sum to the General Hospital, and five thousand dollars each to the Eye Infirmary and the Dispensary of Boston. The donation to the Medical Society is the first it has received from a person not a member. It will be added to the Society's fund for distributing standard medical publications among its fellows.

THE VACCINATION OF INDIANS.—We learn from the National Intelligencer that one of the Senate's amendments to the Indian Appropriation bill provides, to a limited extent, for the continuance of vaccination among the Indians, recently suspended in consequence of the appropriation having run out. The small-pox has carried off thousands of the “ red men of the forest.” By reference to the reports of the office of Indian affairs for the year 1837–8, we learn that this disease swept away the whole tribes of these unfortunate people, and that of the Sioux alone 17,200 died of the disease. More recently, in the year 1853, nearly 12,000 of the confederated bands of the Sioux and Omahas died with the same terrible malady. In 1857, 400 of the Pawnees died from its effects.

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ART. I.—*A Clinical Lecture on Scirrhus, or Hard Cancer.*
Delivered in the Baltimore Infirmary, October 9th, 1860.
By Prof. WM. A. HAMMOND, M. D., Surgeon to, and Lecturer on Clinical Surgery at the Institution.

GENTLEMEN—I desire this morning to say a few words to you in connection with several cases which have recently come under our notice relative to malignant tumors, especially that variety known as scirrhus or hard cancer.

By malignant tumors, I understand those due to a specific constitutional cause, of which they are the local manifestation, possessing an inherent tendency to growth, ulceration and recurrence after removal, and constituted of histological elements wholly, or in part, not normal to the organism. They are, therefore, widely separated from benign tumors, which are essentially local, are not liable to return after extirpation, have no tendency to ulceration, and are formed of histological elements entering into the constitution of the tissues met with in the healthy body.

Rejecting those species of cancer which have been created by systematic writers upon trivial variations, we have four markedly different forms of malignant tumors: 1. The Encephaloid; 2. Scirrhus; 3. Colloid, and 4. Epithelioma—ar-

ranged in the supposed order of their malignancy. All these growths differ from each other in microscopical characters, but all possess, as an essential point in their structure, a peculiar formation known as "cancer cell," and there are many reasons for believing that they are all produced by the same constitutional taint, and consequently are mere varieties of one disease. It is with the second named of these—scirrhus—that I propose to occupy your attention a few minutes.

Scirrhus more frequently attacks the mammary gland of the female than any other structure of the human body, though other tissues, as the skin, the muscles, the bones, the lymphatic glands, the stomach and intestines, the uterus, the ovaries, &c., may be invaded by it. As a type of the disease, let us consider scirrhus as it is found in the female breast.

Scirrhus of the female mammary gland is most apt to make its appearance during the middle period of life, at about the time the catamenia cease to flow. In some cases it would appear to be hereditary; in others, it is referred by the patient to some injury to the gland, but generally no cause can be assigned. Ordinarily, it is first perceived as a small, hard, nodulated lump, generally but not invariably painful on pressure, and movable under the skin. As the disease advances, the gland becomes more and more involved, the irregularity of surface increases, adhesions are formed with the surrounding structures, owing to contraction of the milk ducts, the nipple assumes a brownish discoloration, and the lymphatic glands of the axilla enlarge and become painful. During the progress of the disease, pain is generally present. It is of a sharp, lancinating character, and is frequently compared by the patient to that produced by a stab with a knife. Ulceration and sloughing at length take place, and this, too, before the tumor has reached any considerable size. Instead of pus, the discharge consists of an acrid, excoriating fluid; new granulations spring up, and at length a hard fungus growth is formed, such as you have frequently witnessed in the woman now in the house. Attempts at cicatrization are sometimes made, but they are always futile, and reparation accordingly

never takes place. The fetor is, as you have had abundant opportunity for witnessing, exceedingly great. The general health now, if not before, becomes manifestly involved, the color of the skin changes to a tawny, cadaverous yellow, the features are pinched, and owing to the exhaustive effects of pain, the loss of blood from ulceration of the vessels of the part, and from arrest of nutrition, the constitution becomes broken and death at length ensues at an average period of about three years from the commencement of the disease.

Such is a very imperfect sketch of the progress of scirrhus of the mammary gland.

If we examine a scirrhus tumor of the breast after its removal, we find that it possesses no distinct capsule, but that the tissues of the region are in intimate contact with the new formation. In consistence it is cartilaginous, and when cut into it is seen to be of a general grayish color, with occasional yellow spots and fibres—the obliterated milk ducts. From the surfaces of the sections a thick gruelly fluid may be pressed. This is the “cancer juice.” It consists of cancer cells, oil globules, and fibres floating in the exudation from the cut blood-vessels, lymphatics, and milk ducts. The tissue which remains after the juice has been pressed out is the “cancer stroma.”

The cut surfaces, as Mr. Paget remarks, become concave, whilst in all other tumors they become convex. This is a diagnostic mark of easy application and considerable value.

Let us now glance at the microscopical characters.

The elements of a scirrhus tumor are organic molecules, granules, oil-globules, fibres, free nuclei and fully formed cells. When we examine with a good microscope, with a linear magnifying power of at least 400, the juice from such a tumor, we find all these component parts present.

The elements which are most generally regarded by microscopists as characteristic of cancer, are the free nuclei and perfectly formed cells. The first of these are somewhat larger than those found in the cells. I have met with them having a long diameter of the 1-2000 of an inch. They are

also more irregular in shape. The nucleoli of these free nuclei are also considerably larger than those which belong to the complete cell. Free nuclei are almost always met with in the juice of the scirrhus tumor in very considerable abundance, and I am disposed to regard this circumstance as of much diagnostic value.

In regard to the cancer cell, there is, as you are doubtless aware, very great difference of opinion concerning its importance as a characteristic, and some even doubt its existence altogether. I believe, however, that you will find very few microscopists who deny either the one or the other. The skeptics are not microscopists. Let me point out to you briefly its chief features.

From the form we can learn little or nothing, as its variations in this respect are almost infinite. Generally, however, it is either round or irregularly ovoidal, but you will find many of fusiform, caudate, bi-caudate and various other shapes. This very multiformity is regarded by Bruch, Paget and others as of itself indicative of malignancy, for in none others but cartilaginous tumors is it observable.

The cells of scirrhus tumors vary in size, according to my experience, from the 1-1500 to the 1-500 of an inch in diameter. These measurements are a little larger than those given by Paget. In color they are of a pale gray. They frequently contain granules and a cell wall can generally be detected.

The nuclei are remarkable for their large relative size when compared with those found in other cells. They are about the 1-2200 of an inch in the long diameter, though I have often found them almost completely filling the cell. In form they are almost always oval or round, one or two may be present, and three or even four are sometimes met with. The nucleoli are also large, one, two or three may exist. They are always bright and shining, and this brightness is an important distinctive feature.

The cancer cell is liable to degeneration and atrophy. Frequently we meet with large cells containing numerous oil

globules of small size, and it is not uncommon to find them withered and collapsed.

We come now to the stroma. It is the predominance of this element which gives to a scirrhus tumor its hardness. It consists of fibrous tissue arranged upon no definite plan, in the interstices of which the cancer cells are found. It differs in no respect histologically from the same formation existing in benign tumors. There is, therefore, some plausibility in the view propounded by Vogel, that scirrhus growths are combinations of encephaloid and fibrous formations—the former consisting almost entirely of cell structure.

I have thus, gentlemen, pointed out to you, though imperfectly, some of the chief histological characteristics of scirrhus, characteristics which some of you have had recent opportunities of witnessing for yourselves, and you might ask whether or not they are sufficiently well marked to afford us positive evidence in regard to the malignancy of morbid growths? I should answer emphatically, yes; for I know of no structure but cancer in which they are found. If, however, you were to ask me whether or not I could, from a microscopical examination of a few cells, invariably decide as to the cancerous nature of a tumor from which they were taken, I should reply as positively in the negative. Give me, however, the whole tumor, or at least a considerable portion of it, and if upon microscopical inspection cells were found presenting such appearances as I have described, arranged without any evident tendency to the formation of a definite and normal tissue, or to further development, I should not hesitate to pronounce it malignant, and I do not believe I should run much risk of being mistaken. But, gentlemen, there are none of us infallible, and we must remember not to charge to the microscope the errors of those who use it.

There are several other points which might engage our attention, but I must pass them all over but one, which is too important to be set aside, and that is, the propriety, or not, of extirpation.

You will find among pathologists and surgeons the greatest

diversity of opinion on this subject;—some contending strenuously for extirpation under nearly all circumstances, except when ulceration has ensued; others selecting their cases and only operating when the constitution is good, when there is apparently no hereditary taint, and especially when the glands of the axilla are not enlarged; whilst others, again, who believe the disease to be essentially one of the blood, refuse to remove the morbid mass under any circumstances, contending that it invariably returns, and that life is thereby rendered shorter than if the tumor is left to itself. You would not, say they, remove a gouty toe for the purpose of curing gout.

As is usual in such cases, all sides adduce evidence in support of their positions. According to Paget's statistics, life is shortened even when the operation is performed under the most favorable circumstances; but some recent contributions of Mr. Sibley, which are based upon cases in the cancer wards of the Middlesex hospital of London, show that life is considerably lengthened if the surgeon is careful only to operate when every point is in his favor. We can hardly then regard the question as settled. We require additional statistics. So far, however, as relates to those unfortunate cases in which the lymphatics are involved, or ulceration has occurred, there is no doubt; the operation is never admissible.

When, however, the general health is good, when the lymphatics have not become affected, whilst the tumor is still movable, and when there is no hereditary predisposition—such cases, for instance, as my distinguished friend, the professor of surgery, operated upon a short time since in this theatre—there is a chance that life may be prolonged. I wish I could say more, but I believe the disease will sooner or later return. Above all things, when you operate, be sure that you remove the whole of the diseased mass, and even a considerable amount of apparently healthy tissue. The investigations of Schroeder Van der Kolk have shown that the cancer cells are infiltrated into sound parts to the extent of over an inch from

the circumference of the morbid growth. If one single cell is left, it were better, so far as your patient is concerned, and perhaps also your own reputation, that you had never taken the knife in your hand.

ART. II.—*On Epidemic Jaundice*. By L. FAULKNER, M. D.,
Halifax Co., Va.

MT. LAUREL, *Halifax, Va.*, Oct. 1860.

Editors of Maryland & Virginia Medical Journal: An article in the last number of your Journal from Dr. Pollard, on the subject of "Endemic Jaundice," induces me to communicate to you the fact of the prevalence of the same disease in this county. Not feeling prepared to make a regular essay for "The Journal," I also concluded that by pursuing this course, whatever of interest there might be in my communication could be used by you in any way you thought best towards completing the history of this rare epidemic.

That jaundice as an epidemic is rare, there can be no doubt. Dr. Chapman, that great Virginian, in his work on "Important Diseases of the Abdominal and Thoracic Viscera," mentions its sporadic prevalence under his own observation in Philadelphia, and epidemic among cattle, and refers to accounts of epidemics of it by Dr. Stokes and the elder Frank; but in an article published by him in the "American Medical Journal," vol. 1, Nov. 1st, 1827, seventeen years before the publication of his lectures in this book, there is no allusion to these facts, though the articles are nearly the very same with this exception. Dr. Stokes' articles, in which reference is also made to Frank's epidemics in the years 1742 and '54, were published in same journal Feb. 1835. An intelligent gentleman, who was at Norfolk during our last war with England, (I hope always to be called *the last*,) told me that every man in his company, save himself and one other, had it. And I judge there must have been more or less extensive prevalence of it in this county somewhere about that time, as even old negroes

could tell me of the remedies used for it when they had it on their plantation. So I soon found, that whilst I was thinking upon my first observation of an epidemic tendency in jaundice, that I had met with something entirely new, that Solomon was altogether right when he said "there is nothing new under the sun." Our county, though deprived of the visits of such celebrities as "the Great Eastern" and the Prince, has been visited almost first of all by the celebrated great epidemic diphtheria, and this "royal disease." The first cases I saw were at Halifax Court-house, during the fall of 1857, where there were several cases, and during the ensuing winter. The only death that I recollect from it up to this time, was of the first case, who died, as the attending physician informed me, in an apoplectic condition. I did not encounter the disease again in any number of cases until I removed to this place last fall. I found it during the winter lurking about in this neighborhood, scattering at first, but by spring and summer going almost through large families, and paying its respects to most of the families, leaning rather more to malarial localities. But, as yet, I have not observed it very prominent in the few families, on Difficult Creek, that have had intermittent fever as their "set fast." I have seen it on one plantation in a cluster of houses, and diphtheria in another cluster. This latter epidemic made its first appearance in this region on the south of Dan during the winter of 1856-7, just before jaundice at Halifax Court-house, and succeeded this in the neighborhood of the Court-house sometime during the year 1858. There is no other marked epidemic influence in this neighborhood at this time besides these two; unless it be, according to my own opinion, a wide-spread and all-pervading erysipematous influence existing for several years, and modifying most other diseases.

The jaundice was ushered in with pretty sharp fever, frequently lasting for several days, or until the secretions were aroused, and the usual symptoms were manifested, as sense of weight and oppression about the stomach, loathing of food, &c. I would only suggest that, according to my observation, the first indication of yellow color was always in the urine,

and some gave no other indication. The gentleman who gave me the information of its existence among our troops at Norfolk, happening to remark to me one day that he believed the mud he had recently imbibed in Richmond was just working off by the kidneys, was warned that jaundice was in his system instead of Richmond mud, and sure enough he was so yellow in the course of a week, that he said, upon comparison with a tolerably bright mulatto servant, the latter had the advantage. Yellow vision was complained of in two or three cases, and in one of these the saliva was as yellow as if he had been chewing hickory bark. This case was brought almost to the verge of the grave, more than once, by profuse epistaxis.

Of the pathology of jaundice, which I thought perfectly plain when I read Cullen during my pupillage, I confess my ignorance. I saw, in a case of bilious colic some years ago, the yellow color begin at a point near my cups, I think just over the liver, and spread over the body just as you have seen a shade pass along from a cloud floating across the sun, until the man was completely jaundiced, and that, too, in a few seconds. Irritation of the duodenum must be concerned in its production, and there must be some change wrought in the capillaries and other excretories by nervous influence; or the nerves may be first in this chain, and all moved by a poison in the blood. I almost regarded this epidemic as entitled to a place among eruptive diseases.

The treatment at first, with active mercurials, I found soon to be useless, if not injurious. Indeed, I should avoid it as a poison when diphtheria was co-existing. I pushed it in one case to ptyalism, and my patient got worse and worse, although the liver seemed to pour out bile freely. It ran its course almost as some of our eruptive fevers, susceptible of guidance by mild treatment. Latterly I give only a dose or two of mercury, (calomel and bicarb. soda preferred,) with Dover's powder or opium when demanded, and then order the bowels to be kept well opened, generally with pills of castile soap, aloes and rhubarb, adding ipecac when not forbid by excessive

vomiting, allowing the use of hard cider, infusing in it barberry bark in some slow cases, and a mild nutritious diet. Avoidance of wet and cold and tonics during convalescence. Counter irritation over the epigastrium was required in some cases. I tried the remedy praised by Dr. Chapman as nearer a specific than any other, but the gentleman, the same who compared notes as to color with his servant, took only one dose, and said he felt soap-suds foaming up in his throat all day. One other gentleman, after giving the only blue pill prepared for himself to a friend that he thought needed it more, used no other medicine but a frequent dram, for which he had an excellent appetite. Many persons, without the advice of a physician, used Castile soap in spirits I suppose, or barberry bark, upon the recommendation of older persons, who recollected the treatment when it prevailed in their younger days.

P. S.—In saying that I should avoid the treatment with active mercurials as a poison when diphtheria was co-existing with it, I mean the free use of this potent drug. There is nothing in regard to diphtheria, this so-called new disease(?), that I am better satisfied of, than the fact that mercury is very injurious, given in repeated doses.

ART. III.—*Excision of the Clavicle.* By WM. M. FUQUA,
M. D., Richmond, Va.

AN Irishman, aged 40, of good health, presented himself at my office on the 7th of Oct., with a carious clavicle.

The original cause, from his account, was from carrying a heavy load of tools upon his shoulder, thereby bruising the integument over a portion of the collar-bone. He is a rock mason by trade. It was not long before he had pain along the clavicle. Soon it began to discharge pus, and after the lapse of several months there was a solution of continuity. The outer two-thirds of the bone was enlarged to a considera-

ble extent, with an unhealthy cicatrix along its middle third. The inner extremity was diseased to within three-quarters of an inch of its sterno-clavicular articulation, as was found after removal of the bone. I determined to remove the entire bone, which was accordingly done on the 10th of October: Drs. Peticolas, Crenshaw, Clopton and Brock being present. I first placed him under the influence of chloroform, which, by the by, so tranquilized respiration as to assist me very materially in the operation. The shoulders being elevated, an incision, beginning at the sterno-clavicular articulation, was carried along the line of the clavicle to its outer extremity. The pectoralis major was then severed from its attachments and turned back. Along the upper border of the bone a few fibres of the sterno-mastoid were divided and the dissection proceeded superiorly until the platysma and trapezius were detached. On turning these back, the bone was exposed; then disarticulated it at its extremity (outer). In like manner, the inner extremity was exposed; not all of it, however, was found diseased. It was determined, therefore, to remove only the carious part. This was done by passing a chain saw beneath the fragment, and with a few strokes it was removed. Having cleansed the wound thoroughly, the extremities were sutured, whereas the intervening portion was held together by adhesive strips. He was then put to bed.

Oct. 10: Expresses himself as feeling badly, face flushed, pulse 90; slept none last night. —Morph. sulph. gr. $\frac{1}{4}$.

Oct. 11: This morning, he says, he is much better. Shoulder painful and considerably swollen; removed sutures; union had taken place at either extremity. Daily the wound injected with dilut. chlorinated soda, and adhesive strips applied.

He is now walking about, the wound having almost entirely healed.

ART. IV.—*Reports from the Infirmary of the Medical College of Virginia.* By THOMAS L. HUNTER, Resident Physician.

CASE I.—*Excision of two-thirds of the Radius.*

Delaware; a negro boy; aged 14; with necrosis of the radius of the right arm.

This bone has been diseased for many years, and “sequestra” have been from time to time discharged through several ulcers that are scattered over the lower part of the arm. The disease was developed without any existing cause. It is evidently scrofulous in its nature.

Aug. 6: This morning Dr. A. E. Peticolas, the attending surgeon, excised the greater portion of the radius. An incision was made beginning about an inch and a half below the elbow joint, and carried to within an inch of the wrist, in a line with the thumb and external condyle of the humerus. The bone was then thoroughly exposed, and cut through at its two extremities with the chain saw, removing the middle two-thirds, leaving two inches at the upper and one at the lower extremity, the whole of the diseased part being included in the portion removed. Tents of lint were placed in the wound and cold water dressings applied.

Aug. 7: The arm is swollen and painful. *Ordered*, Continue the same dressing. R.—Morph. sulph. gr. $\frac{1}{4}$.

Aug. 8: A high grade of inflammatory action is set up in the arm, which has already advanced to extensive suppuration in the wound, with sloughing of portions of the divided tendons. *Ordered*, Apply a fermenting poultice. R.—Tinct. ferri chlor. gtt. xv.; quinine sulph. gr. ij. Sig. ter in die.

Aug. 12: The wound is looking better. All sloughing has ceased. Still suppurating. Very offensive. *Ordered*, Stop the poulticing, and wash the wound daily with a solution of chlorinated soda. Apply cold water dressing; place the forearm in splints in a semi-prone position, and suspend it in a sling.

Aug. 20: The patient is doing well. All undue inflammatory excitement has subsided, leaving a healthy granulating

surface, which only requires an occasional touching with nitrate of silver, at the same time that the water dressing and splints should be continued. On the 20th September he was discharged cured. Pronation and supination could be performed with ease, and the movements of his fingers were as perfect as before the operation.

REMARKS.—In this operation the surgeon resorted to an expedient that was alike ingenious and effective.

In the beginning the arm was elevated and allowed to remain in that position for several minutes. A bandage was then tightly applied from the tips of the fingers up above the elbow joint, and a tourniquet firmly adjusted at the point where it ceased. The bandage was afterwards removed, and the operation proceeded with in the usual way. By means of this preliminary precaution, the limb was entirely exempt from venous congestion, and the surgeon was enabled to perform his work unembarrassed by the venous hemorrhage, that would otherwise have welled up and filled the cavity of the wound, and required the constant application of the sponge to remove it. In this case, the loss of blood was very trifling, probably not exceeding two ounces.

The Doctor mentioned that he had in several instances resorted to this plan of bandaging the limb before the tourniquet was applied, with the happiest effect in cases of amputation of the leg, where there was much danger to be apprehended from the loss of even a few ounces of blood.

CASE II.—*Operation for Strangulated Scrotal Hernia.*

Jacob; a negro man; aged 30; admitted Aug. 2d, 1860.

This man has had reducible scrotal hernia for two months. It descended yesterday, and became strangulated. He was admitted at an early hour this morning, suffering intense pain in the abdomen, with frequent stercoraceous vomitings. Dr. A. E. Peticolis, the attending surgeon, failing to reduce the hernia by taxis, operated at 10 o'clock, and replaced the protruding gut.

He took before and after the operation three doses of morph. sulph. gr. ss.

Aug. 3: Slept well last night. Has little or no pain except when he coughs. Has had a move from the bowels. *Ordered*, Morph. sulph. gr. $\frac{1}{2}$; to be given at bed time.

Aug. 4: The wound is doing well. Bowels are rather loose. Has had eight passages during the night. *Ordered*, R.—Plumbi acet. gr. iij.; pulv. opii gr. j.; *M. ft. pill. no. j.* Sig. To be given at once, and repeat if necessary.

Aug. 6: Free from all bad symptoms. Very comfortable. *Ordered*, Give morph. sulph. gr. $\frac{1}{2}$, at night.

Aug. 7: The sutures were removed this morning; union is complete throughout the whole extent of the wound. *Ordered*, Stop the use of the water dressing. Give no more morphine. Place a compress over the wound and strap tightly and apply a suspensary bandage.

Aug. 8: Renew the same dressing. The wound is healing very rapidly. No discharge. Had one passage from the bowels last night; caused no pain. Says he feels as well as he ever did in his life, with the exception of the fatigue he suffers from having to lie so constantly on his back.

Aug. 12: Perfectly comfortable. Wound almost cicatrized. Bowels regular. Sleeps well without an anodyne. A compress and tight bandage is applied, and he is allowed to change his position in bed.

Aug. 24: A week ago he was up walking about the ward; but within the past few days a tumor has formed, extending from the internal ring into the scrotum. It is very hard and large, increasing in size from above downwards. The cord is involved in the tumor, but the testicle is not. It causes little or no pain, other than a sensation of tension. The patient is ordered to be kept quiet in bed, and a poultice to be applied.

Sept. 3d: This morning the tumor opened spontaneously and discharged nearly a half pint of pus. The abscess pointed in the recently formed cicatrix. For the past day or so he has complained of much pain in the part, but now he feels easy. *Ordered*, To continue the poultice.

Sept. 9: The abscess has ceased discharging. The tumor is much smaller and softer. *Ordered*, Stop the poultice. Apply argt. nit. to the granulating surface and use simple cold water dressings.

Sep. 25: He was discharged perfectly and radically cured.

ORIGINAL TRANSLATIONS.

I. *Abridgment of a "Report upon facts in Diphtheria, observed in the service of Professor Trousseau during the first half year of 1859. By Dr. EUGENE MOYNIER, Chef de Clinique at the Hôtel Dieu, Paris."*

DURING the first half year of 1859, M. Moynier observed twenty-nine cases of diphtheria in the service of Prof. Trousseau. Diphtheria did not always appear to us under the form of a false membrane; in a certain number of patients, its previous existence was ascertained by its effects and consequences. In *twelve* instances, it assumed the laryngeal form, and constituted croup, giving rise to symptoms grave enough to call for the operation of tracheotomy, which was done *eleven* times and followed *thrice* by recovery. Once it was deemed useless, and the child died without the operation.

In *six* cases the disease was confined to the pharynx without involving the larynx; the patients were older than the preceding; out of six, one died and five recovered.

In *four* cases it assumed the cutaneous form; and this was in very small children, from seven weeks to three months old. Of these, one died—the three others recovered.

In *five*, albumen was found in the urine. Finally, in *eight* cases, diphtheric paralysis occurred.

The author takes up separately each of these four forms :

1. *Laryngeal diphtheria or croup*. Eleven children attacked with croup, entered the Hôtel Dieu ; a twelfth was treated in the city. Of these twelve, seven were girls and five boys ; among the former there were two cures, and one only among the latter. The boys were aged : one 14 months, one 18 months, one $2\frac{1}{2}$ years, two 3 years ; the girls were, one 18 months, one 3 years, two 5 years, two others 6, and one 16 years. Two of the children cured were 3 years and one 5 years old.

These facts show how rare recovery is in children of less than two years ; and it will presently appear that croup is more infrequent under 2 years than from 3 to 8 years, and that diphtheria assumes the cutaneous form in very small children.

We gather, says the author, a very incomplete account of the phenomena presented by patients before their admission into the hospital ; our children often reach us without having had any treatment. This circumstance would tell favorably upon the result of tracheotomy, if, at the same time, we could choose the moment for operating ; but the little patients were sometimes brought under our care in such a condition that the surgical interference offered no promise of success. In these extreme cases the operation is not followed by amelioration even ; the children seem as if poisoned or asphyxiated, and their vitality to be destroyed. Thus, young children, who had presented no trace of cutaneous diphtheria, (general poisoning,) die after tracheotomy, sinking under their protracted asphyxia ; and the autopsy reveals only false membrane in the larynx, unaccompanied by any inflammatory lesion of the pulmonary parenchyma. These children appear not to suffer ; the face is not livid, respiration is not difficult—they expire by annihilation of the vital forces.*

* “ An animal placed under a glass bell, vitiating the enclosed air by his respiration, falls at length asphyxiated. Now introduce under the bell another animal of the same strength and species ; the latter seems to be immediately overwhelmed, and is much more oppressed than the former. If we

The accession of diphtheria could not, of course, be studied by M. Moynier in cases brought from without; but he instances seven cases in which sore throat preceded the suffocative periods from three to thirteen days, and one in which the disease supervened immediately upon measles.

Etiology. In 8 of 28 cases of diphtheria the cause was unknown. Thrice it was attributed to cold; and, in twelve cases, the influence of contagion is claimed. "Among the observations of Dr. Faure upon diphtheric paralysis are to be found these facts. A child dies of a pseudo-membranous affection of the respiratory passages; its brother takes the same disease, but is cured; the mother of these children falls sick, and then a friend who sat up with the dead child, and the wife of this friend and her sister are seized at the same time with a pseudo-membranous angina, which were followed by paralysis. * *"

M. Moynier saw a nurse of the Hôtel Dieu who contracted a diphtheric angina while waiting upon children affected with croup.

Marie B., who died at 13 months of a cutaneous and pharyngeal diphtheria, had played, a few days before, with a child laboring under fibrinous angina.

G., dead of croup, had lost two brothers by the same disease, &c., &c.

Treatment, for want of opportunity, was rarely instituted in the earlier stages. At later periods, it consisted in emetics of sulphate of copper, insufflation of alum, or alum and tannin, or gargles of alum and honey.

Finally *tracheotomy*, for which the indications are: "very difficult respiration; the production, during inspiration, of a deep depression in the epigastrium; attacks of orthopnoea; incomplete hematoxis; lividity of the lips and face, and extinction of the voice and cough. When these symptoms are present, tracheotomy is necessary, even excluding pseudo-membranous croup."

restore them to the air, the second recovers his animation almost immediately; the other rallies more slowly, and sometimes dies after a little while."—*Experiment of M. Claude Bernard*.

[We omit a case of "laryngeal diphtheria," in which tracheotomy was successful, as well as a description of Trousseau's well known method of performing the operation; but give the statement of M. Moynier, that out of 25 operations of tracheotomy which he attended, there were 11 recoveries, 8 out of 20 in the hospital, and 3 out of 5 in the city.]

2d. *Pharyngeal Diphtheria*.—This form, by its conduct, creates less apprehension than laryngeal diphtheria or croup, for suffocation is not pressing, strength is exhausted, and the patient does not seem to suffer. However, this security is mischievous, for although the danger is not so imminent as in croup, it is nevertheless "very real." Thus, out of six patients with pharyngeal diphtheria, one died from angina itself; and besides, it is, perhaps, that form of diphtheria which is most frequently accompanied with paralysis, and this may eventuate fatally.

Six patients had pharyngeal diphtheria; four recovered, two died. The false membranes sometimes occupied the whole extent of the pharynx, the tonsils, palate and uvula; frequently the entire mucous membrane of this region was completely covered with it. Portions of it three-fourths of an inch or one and one-eighth inches might be detached with forceps, and thus was removed from the uvula a complete cap which had enclosed it. Patients also ejected tubules of false membrane, a ramification when the disease extended into the smaller bronchi. The thickness of these diphtheric patches was sometimes considerable, but diminished as the affection retrograded.

In grave cases of pharyngeal diphtheria the cervical and submaxillary glands are always enlarged, and their augmented size follows the progressive increase of the false membrane, or their volume lessens with the diminution of the latter. The prognosis of pharyngeal diphtheria, however, should be based rather upon the condition of the ganglions than upon that of the false membranes.

Another symptom bearing upon the gravity of the disease

is albuminuria. The author has found albumen in the urine whenever the diphtheric membrane was extensive. It indicates that the economy is deeply affected; and where diphtheria did not appear very threatening, the patients were subsequently attacked with paralysis.

As contagion is admitted as a cause of the disease, prophylaxis must evidently consist in sequestration. The curative treatment comprises tonics, as cinchona, coffee, &c., good diet, and the application of caustic or astringent substances to the mucous membranes of the throat, denuded as much as possible of the false membrane. Among the topical applications chlorohydric acid stands first, but nitrate of silver, alum and sulphate of copper are very useful, either as adjuvants or employed when the false membrane is of small extent. The perchlorate of iron in concentrated solution has disappointed the expectation of M. Trousseau; but its value as a tonic is freely admitted.

3d. *Cutaneous Diphtheria*.—By this term the author means to express “general poisoning—and as redness and swelling of the amygdalæ precede exudatory angina; and as laryngitis ushers in croup, so redness, excoriation of the skin and denudation of the dermis by an excoriation, an eczema, a blister are the conditions necessary for the production of cutaneous diphtheria. An excoriation which, under ordinary circumstances, would be harmless, will, if the bearer of it be exposed to the atmosphere of diphtheria, be rapidly covered with false membrane. It is not asserted that the skin only is affected, although this organ sometimes is the exclusive seat of disease; and the false membrane arising upon a blister or wound of any kind invades the tissues deeply and widely. This affection may exist alone, or may precede or be followed by tracheal or other diphtheria. Of four cases observed, one died; in this case the air passages were deeply involved.

The treatment is essentially the same as for pharyngeal diphtheria, with the addition, however, of a poultice of bread and milk to the inflamed part, and a dressing of “*calomel* pomade.”

4th. *Diphtheric Paralysis*.—This affection, first recognized by MM. Bretonneau and Trousseau, is full of interest. It usually follows closely upon the ordinary symptoms of diphtheria, or even occurs before their subsidence, the first indications of its approach being a difficulty of swallowing and a nasal twang in the voice, and then the legs and arms lose power. But, on the other hand, the paralysis may not be observable for a month or two after the cure of diphtheria. After tracheotomy, children are seized with dysphagia, a consequence of this paralysis, and are liable to suffocation from the occlusion of the trachea by morsels of solid food.

The paralysis begins in the velum palati, then the extremities are affected on both sides, constituting a general adynamia on one side, giving rise to hemiplegia (involving one-half the bladder even), or the lower extremities, occasioning paraplegia. Patients experience formication or numbness; many have incontinence of urine and fæces; and may even suffer loss of vision. It is characteristic of these paralyzes that the phenomena of contractile excitation in the muscles involved are absent.

In all the men laboring under diphtheric paralysis there was anaphrodisia. They all stated that they had no erections, experienced no amorous inclinations, and rarely had pollutions. The memory fails, impressions are ephemeral, reflexion is slow and laborious, and the intelligence is dull.

Albumen is found in the urine in the great majority of cases, and is the expression of a general modification deeply affecting the economy; indeed, M. Trousseau compares the action of diphtheria and its consequent paralysis to a poisoning.

The diagnosis of the paralysis depends upon the antecedents of the patients, the previous existence of diphtheria being the best indication, and then the manner of its invasion, as just given. In this connexion, M. Duchenne de Boulogne has established an important fact, which is, that in diphtheric paralyzes muscular contractility remains intact, while it is abolished or diminished in paralyzes symptomatic of disease of the spinal marrow.

The treatment is principally tonic, and consists of good diet, cinchona, citrate and per-chlorate of iron, and sulphuretted and saline baths. In addition, frictions are to be employed along the spine and on the limbs, and electricity is sometimes efficacious. But the most effectual remedy, which acts generally by stimulating the appetite and exciting all the vital properties, and upon the local condition by reviving the muscular contractility through the contractions that it sometimes occasions, is strychnine, which is of great service in the affection under consideration. Nevertheless, it must be confessed, that in spite of all these therapeutic resources, the disease will last for a certain period, and time alone eventually overcomes it.

C. J.

II. *Irritating Substitutive Medication*. Translated from Trousseau and Pidoux, vol. 1, p. 470, et seq. By D. I. McKEW, M. D., Baltimore.

THE doctrine of Homœopathy, considered in the general idea upon which it is based, certainly does not merit the ridicule which its application to therapeutics by homœopaths have drawn upon it. When Hahnemann evinced the therapeutic principle *similia similibus curantur*, he proved his *dictum* by the support of facts drawn from the practice of the most eminent physicians. All evidence proves the frequent cure of local inflammations by the direct application of irritants, which cause an analogous inflammation—a therapeutic inflammation substituted for the primitive irritation. That which was true of local diseases and topical agents was certainly much less so in the case of general affections and general remedies; but Hahnemann, seduced by the truth of an idea of which he had caught a glimpse, and which he had reduced to a formula, like all innovators, very soon exaggerated the importance of his discovery. His disciples, as is always the case, outstripped

their master, and drew him into their exaggerated ideas; and German mysticism soon becoming mixed up in the matter, homœopathic therapeutics became sufficiently singular to attract numerous partizans; for there is no idea so absurd as not to find medical men to sustain it, and patients to offer themselves for experiment. Homœopathy has had its day in France as everywhere else; there is scarcely a physician who cannot accuse it as the cause of some defections among his patients; but as the excitement has now passed and there is no motive to enter the lists with an enemy disarmed by ridicule and failure, let us try to discover what there is really practical, not in the therapeutic reveries of ancient homœopathy, but in the first effort of Hahnemann while in the vigor of youth. When a morbid cause is applied to the human body, the effects determined are necessarily in relation with the nature of the cause which acts, and with the state of the economy which receives the impression.

Nature of the Cause.—According to Broussais and Brown there is but one morbid cause—the application of excitants to the body. All causes act only by the greater or less degree of excitation which they provoke; excitants being the cause, excitement follows as the effect; this is pretty nearly the substance of the pathological doctrine of these two great innovators. Difference of intensity in the cause, and difference in the mode of re-action of the economy, are the sources of the innumerable differences in the appearances of disease. The different interpretations put upon the varieties of re-action by Brown and Broussais are the cause of the wide discrepancy between the therapeutic conclusions at which each of them arrived. And nevertheless the fundamental idea of their doctrine is identical; Broussais has acknowledged it in taking the synthetic proposition of Brown's doctrine as the text for his own. As we have already said, Brown and Broussais admit as an axiom an erroneous proposition which nothing has hitherto sustained; and as all their doctrine is based upon this frail foundation they have, though reasoning pretty closely and with some show of logic, arrived at the most false conclu-

sions. To say that *life is maintained only by stimulants*, is to enounce a proposition which, at first sight, apparently true, will appear upon a moment's reflection incapable of proof. It cannot be denied that life is maintained by modifying agents; this is a self-evident proposition, but precisely because it has the triteness of an axiom it is good. By the use of the words *modifier* and *modification* we merely express facts; by *stimulants* and *excitement* we substitute opinion for facts, and reason badly. It is a fatal error in philosophy to attach but a moderate importance to words. In principal propositions, words are sacramental, and should be so definite in their meaning, that their proper appreciation shall never be a source of hesitation to the reader. We shall see further what philosophic importance we must attach to the words *modifier* and *modification*, and how much better they square with the facts than does excitation—the principle of Brown and Broussais. For these two pathologists the quantity of the stimulus is everything, the state of the organism being supposed the same in all men. The cause of the gravity of a pneumonia is *cæteris paribus*, the quantity of stimulus applied to the pulmonary parenchyma. The extent of the local lesion, a proportional excitement, tallies exactly with this quantity. The same holds good for pleura, pericardium, peritoneum, liver, uterus, brain, &c. &c. The truth of these facts cannot be denied, and it is precisely such evidence which has seduced and misled the illustrious physician with whose ideas we do not coincide. But when they were shown the different results produced by exterior and physical influences, evidently the same, for all they appealed to the differences of organization, and in this they acted reasonably. Here was evidently a large proportion of diseases entering vigorously in the circumscription of their system. But they were soon disturbed by those pathologists who gave themselves up to the study of special diseases; and it should be said that *Bretonneau* specially, in calling the attention of physicians to the special lesions of the mucous membrane, particularly in diphtherite and dothinerterite, inflicted upon the schools of Edinburg and Val-de-Grâce a blow, the force of

which Broussais vainly tries to conceal. To the mind of Bretonneau and to ourselves, differences in the nature of the cause produce in diseases much greater differences than do the varieties of organization. In our opinion, then, it is not the *quantity* but the *quality* of the morbid agent which determines the nature of the disease: just as it is not the *quantity* but the *quality* of the seeds which determines the species of the product. Quantity gives only *more* or *less* as a result; quality determines the kind. There is, now-a-days, no pathologist, how infatuated soever he be with the dichotomous doctrine, who does not admit some local or general disease, the forms of which are so constant, so invariable, that one is forced to recognize the importance of the quality of the modifier; but these diseases are for them the exceptions—for us they are the rule. In the first rank of special diseases we must certainly class the contagious diseases, without exception. Of these we can truly say, that the seed is sown, and that they necessarily retain the quality of the generating agent. But the number of contagious diseases exceeds general belief, and many catarrhal affections, usually regarded as simple and very ordinary, are transmitted from the sick to the well. But excluding the idea of contagion, and judging disease only by the general and local phenomena alone, we shall see that the class of special affections will at once take a position to fill the greater part of the nosology. Taking, then, as examples those modifiers only, the effects of which are most easily recognized, we see in the case of chemical agents, action special to each resulting from their contact with the human frame. Thus the chlorides of gold, arsenic, zinc and antimony; fire, caustic potash, soda and lime; nitric, sulphuric, hydrochloric and fluoric acid; the salts of silver, copper, mercury, &c., &c., produce upon the skin such different action, that with a little experience the particular modifier in each case may be diagnosed by the manner in which it behaves toward the tissue, as a chemical or physical agent, as well as in the form of local re-action which follows the application of the modifier.

It is evident that we cannot here reason from the quantity

of the perturbing agent; for experience shows that we will never produce with nitrate of silver the effects of butter of antimony in whatever quantity we employ it. That this result depends on the chemical properties of the two modifiers and upon the manner in which they combine with the parts, is of little matter, provided there be a difference, and that that difference is constant. If we now examine the poisons we shall see each one acting in its own way, and so strikingly, that a slight study suffices at once to indicate the nature of the poison. There is certainly no toxicologist of even slight experience, who cannot distinguish between poisoning by opium and that by stramonium, veratrine or strychnia: who cannot seize the distinction between the effects of lead and those of mercury, copper and arsenic, or who would fail to recognize the diversity of accidents resulting from the absorption of the poison of the crotalus, the viper, the scorpion, the tarantula, the bee, the mad dog, or a farcied animal. Here each cause has a special effect, characterized by a particular form, which is always reproduced just as the specific characters of a series of individuals constituting a species in a common genus.

(To be Continued.)

SELECTIONS.

I. *On the Natural Constituents of the Urine.*

[Urinary investigations continue to be prosecuted with ardor by the profession. We find the leading articles in both the Dublin Quarterly and Edinburgh Monthly, for August, upon this subject. The one from the pen of the Rev. SAMUEL HAUGHTON, F. R. S., fellow of Trinity College, and the other from that of Dr. WILLIAM SELLER.

It is a pleasing field of study to enter, because it admits of

being approached by so many avenues, and requires so much ingenuity to follow in all its complexities. So far as researches have gone, it has proved to be equally fruitful in facts and beset with errors. Its study requires chemical knowledge, calculating facility, and a familiarity with physiology and pathology. No one subject relevant to the human body, can be entirely isolated in study or practice, or subjected to analysis as a detached unit; but less than most others that of the renal secretion. Complex in its nature and dependent for its complexity upon (we might almost say) innumerable causes, its study reveals to us a history of the life, and often-times the cause of death of the individual.

The two articles mentioned are examples of the truth of these remarks, and without further comment we will give the conclusions arrived at in them.

The articles are filled with minute detail and are rather suggestive than conclusive. Mr. HAUGHTON'S is "On the Natural Constituents of the Urine of Man," and a comparison is made of the urine "with the daily work, bodily and mental, and with the daily food of man." He says:—EDS.]

"I take for granted that the substances excreted by the urine of a healthy man result from the wear and tear of tissue of every kind in the body; and that, in a state of health, this excretion is exactly compensated for by the assimilation of an equal amount of the same substances ingested.

This hypothesis would be readily granted, if it included all the *excreta*—per cutem, per halitum, per anum, per vesicam, provided the *ingesta* also included all received by the stomach, lungs (and skin?)—but, in restricting my hypothesis to the urine, I was aware that I differed from many physiologists, and that the *onus probandi* rested upon me. I, accordingly, undertook a number of observations on the food taken by the persons experimented on in the preceding part of this paper, particularly with reference to the nitrogen received in their food compared with the nitrogen excreted by the urine. The results of these observations I shall now detail, previously to deducing any inferences from the excretions of the urine. I determined the water lost by drying each article of food at 212° F., and afterwards analyzed the dried product for nitrogen, by burning in a tube with soda lime, and collecting the

ammonia in muriatic acid. The quantity of nitrogen was found by adding bichloride of platinum, and afterwards weighing the double chloride of platinum and ammonium on a weighed filter; or by the weight of platinum after ignition; or by both methods. I always found, when the ignition of the double chloride was conducted slowly, that the results were identical with those given by the direct weighing of the ammoniaco-platino-chloride."

The experiments and calculations are then minutely described and given together with the following "conclusions deduced from the preceding discussion:"

"1. The quantity of urea passed per day by men in health varies with their food and occupation, the latter being the principal cause, and regulating the other.

2. Men employed only in manual or routine bodily labor, are sufficiently well fed on vegetable diet, and discharge on an average 400 grs. of urea per day, of which 300 grs. are spent in vital, and 100 grs. in mechanical work. This conclusion is in conformity with the experience of the mass of mankind employed in manual labor in all ages and countries.

3. When the work is of a higher order, a better quality of food must be supplied, sufficient to allow of a discharge of 533 grs. per day of urea, of which 300 grs., as before, are spent in vital work, and 233 grs. in mental work and the mechanical work necessary to keep the body in health.

4. The quantity of urea discharged per day varies also with the weight of the individual, which influences the vital and mental work.

5. The habits, weight, and occupation of the individual enable us to account for a range of the diurnal quantity of urea, varying from 300 to 630 grs. per day; and this discharge may be confidently predicted, when the habits and weight are known.

When, in any case, the discharge of urea exceeds that calculable from the preceding data, it must be attributed to ill health, and most generally to that most fatal of all diseases to which man is liable—anxiety of mind—a vague and unscientific expression, which, however, denotes a most real disease.

This fact alone would render the preceding investigation of importance to the physician, as it enables him, in a given case, to pronounce whether there is an excess of urea or not, and a consequent waste of the system. I have shown that the mere quantity will not decide this question, as from 300 to 630 grs.

may be discharged by persons in perfect health, according to their peculiar work and physical conditions."

Dr. SELLER's paper is "On the Determination of the Proportion of Solids in the Urine of Health and Disease." He says:

"Of late, my attention has been drawn to the modes of estimating the proportion of solid matter in the urine. There are still attendant on the reduction of the urine to dryness, great practical difficulties; and, respecting the proper rule for solving the problem by reference to the density of the urine, there are conflicting views among the best authorities.

It will be remembered that the three best-known proposals for finding the proportion of solids in the urine from the density are, to multiply the number denoting the excess of the density above 1000, 1st, by the number 2; 2dly, by a number less than 2 (1.65); 3dly, by a number greater than 2 (2.33).

This mode of proceeding rests on the circumstance, that when soluble matter is added to water, the solvent commonly, yet not always, undergoes an expansion.

If there be no expansion, there is no need of a multiplier—the excess of the density above 100 signifies the amount of the soluble matter present. If the expansion expressed in water-grain measures be found to be equal to the excess of the density above 1000, represented by weight in grains, then the multiplier 2 will give the amount of soluble matter contained in the solution. For example, in the 1000 grain specific-gravity bottle, let 50 grains of soluble matter, the mean expansion caused by which is 25 water-grain measures, be dissolved in 975 grains of distilled water, the bottle will be exactly filled, and the specific gravity, which here corresponds with the actual weight, will be 1025. But 25, the excess of the density or specific gravity above 1000, multiplied by 2, gives the whole amount of solid matter, viz., 50 grains. Parallel to this is every case in which the expansion is equal to the excess of density above 1000. And thus, that form of the rule under consideration which makes 2 the multiplier, proceeds on the assumption that the number denoting, in water-grain measures, the mean expansion caused in water by the solid matter of the urine is equal to half the amount of that matter expressed by weight in grains.

When, again, 1.65 is used as the multiplier, the method proceeds on the assumption that the expansion is to the excess of density above 1000, very nearly as 2 to 3, or is equal to two-fifths of the number denoting the weight of the soluble matter present.

Lastly, when the multiplier is made 2·33, the assumption exists that the expansion is to the excess of the density above 1000, nearly as 4 to 3, or is almost equal to four-sevenths of the number denoting the whole amount of solid matter present.

Of the practical rules before referred to, that most in favor in this country at present is the one proposed by Dr. Christison, not for the urine in general, but for the special highly dense urine of diabetes—that, namely, by which 2·33 is made the multiplier. While many adopt this rule implicitly, like Dr. G. Bird, who has founded two extensive tables upon it, others qualify it by the rule wherein the number 2 is made the multiplier, which they recommend when the density of the urine is lower in the scale, while they take Dr. Christison's rule only when the specimen of urine under examination is of high density. The third rule, that in which 1·65 is made the multiplier, was proposed by the eminent French observer, Becquerel. This last rule, however, has been, of late years, thrown entirely out of English books on the authority of Dr. Day, who, in an elaborate paper, has laid down Dr. Christison's rule as that most applicable to all kinds of cases.

The evidence on which each of these rules has been supported by their respective partisans, is the actual amount of solid matter obtained from numerous specimens of urine of known density. The objection to this evidence is the discrepancy in the results described by different observers—which discrepancy is manifestly dependent on the difficulty of getting the solid residue perfectly free from water.

I am not aware that any one has hitherto attempted to try the merits of these several rules by the particular properties of the known constituents of the urine. This kind of inquiry, however, does not appear unworthy of attention.

The solid matter of the urine in health admits of a threefold division, namely, into—1, urea; 2, organic matters other than urea; and 3, inorganic saline substances. The proportions under these several heads, in many standard analyses of the urine in health, approach to an equality. Thus, in the analysis given by Miller in his *Chemistry*, the proportion of urea in 1000 parts of urine of specific gravity 1020, is 14·23; that of organic matters other than urea is 15·56; and that of inorganic saline substances is 13·35; while the proportion of water amounts to 956·80. Thus it may be allowed to us to assume, as a convenient diagram of the constitution of the urine, that the proportion in each of these divisions is equal, say 15 of each in the thousand parts. Such a diagram will stand thus: Urea, 15; organic matters other than urea, 15; saline substances, 15; water, 1000, less the amount of expansion; while

the specific gravity will hardly exceed 1020. It is obvious that, were the medium expansion created in water by such a proportion of each of those divisions ascertained, a considerable step would be gained towards determining which of the multipliers before referred to should in general be preferred.

With respect to the determination of the proportion of solid matter in the urine by evaporation to dryness, it has long been manifest that the difficulty of the ordinary methods place them entirely beyond the reach of medical men for practical use. A mode, however, occurred to me some time ago, which I have repeatedly put to trial. This mode is, I think, susceptible of very great accuracy, and at very little cost of time. It must be confessed, nevertheless, that without due precautions, it may lead into very considerable errors. It is founded on so simple an observation as that common filtering paper readily takes up a large proportion of urine, and, under favorable circumstances, dries in no long time. The only considerable source of error is the difficulty of bringing back the paper to exactly the same hygrometric state in which it was at the beginning of the experiment. The paper is to be carefully weighed before being moistened, and weighed again with the same care after it has become dry—the difference in weight is the amount of the solid matter in the quantity of urine employed. For 1000 grains of urine, or rather for 1000 water-grain measures of urine, I have commonly used three sheets of filtering paper, cut each into four pieces. The urine is poured into a well-dried basin, and each piece is moistened in succession, as far as it will bear without dripping; enough being left unwetted at its upper part to allow of a fold, by which it may be hung on a string or wire to dry. For this effect a few hours suffice. If the temperature and hygrometric state of the apartment can be kept uniform during the whole process, there is plainly no difficulty; if the drying be carried too far, there is an error of deficiency in the amount of the solids inferred; if the drying be insufficient, or if the apartment has declined in temperature, or its hygrometric state has become greater, there is an error of excess. It should be remembered that the paper, though apparently dry, readily absorbs moisture, owing not only to its original tendency, but to the deliquescent character of the solid matters contained in the urine.

[This was the method proposed and adopted at our Alms-House Hospital fifteen years ago, for the quantitative analysis of the solid constituents of the serum of the blood, by Prof. David Stewart, and the late Prof. Charles Frick.—EDITORS.]

II. *On Paralysis of the Bronchial Muscles.* By JAMES F. DUNCAN, M.D., T.C.D., Fellow of the King and Queen's College of Physicians; Physician to the Adelaide and Simpson's Hospitals.

THE morbid changes occurring in the muscular tissue of the pulmonary system do not appear to me to have attracted that attention from pathologists which has been so readily extended to every other structure in the body. It is quite true, that we have for a very long period been accustomed to recognize spastic contraction of these fibres as a frequent cause of some forms of dyspnœa; and various appropriate remedies have been resorted to for the relief of those diseases, suggested by the idea of their convulsive nature. But the opposite condition of impaired contractile power, if it has not been totally overlooked, has at least been greatly disregarded. No attempt, that I am aware of, has been made to ascertain the peculiar symptoms and signs arising from such a state, or to determine the treatment suitable for its removal or relief. And yet, judging from analogy, no reason can be assigned why such a condition as that of paralysis, or impaired vital energy, should not occasionally occur here, as well as in other muscles; or why, when such a condition does occur, its existence should not be discoverable by a careful observation of its attending phenomena.

Several circumstances naturally suggest themselves as serving to account for this omission. In the first place, the small size and obscure development of these muscles give us the idea that they are to be regarded rather as a rudimentary structure, preserving the analogy of form with the same parts in other animals, than as an apparatus designed for any special use in the economy of man. Very few practical physicians, I believe, look upon these muscles as performing any efficient part in healthy respiration; and hence they are not prepared to expect any material inconvenience to be produced as the result of their impaired action. In the second place, the fact that care has been taken, by the very organization of the parts, to prevent the respiratory process being materially interfered with by morbid changes in the vital action of these fibres, renders the occurrence of these changes, when they do take place, less obvious to the general observer. Not being voluntary muscles, neither the patient nor the physician is placed in a position to discover the loss of their contractile energy, while the manner in which they are associated with the elastic structures of the lungs is calculated to counteract the injurious effects that would

be sure to follow, were collapse of the air-passages a necessary result of their paralysis. In the last place, morbid changes in the muscular structure generally are not of a nature to be recognized by microscopic examination. Changes in other tissues are too obvious to be overlooked. The mucous membrane may be congested, or thickened, or stripped of its epithelium. The serous membrane may lose its transparency, or be covered with an adventitious layer of lymph. The air-cells may be ruptured, enlarged, or otherwise altered. The parenchyma of the lungs may be hepatized, infiltrated with purulent matter, or œdematous, all of which can be easily recognized by the senses. But the muscular structure seems to defy alike the scalpel of the anatomist and the lens of the microscopist. Even atrophy, a condition, with the occurrence of which in other muscles every pathologist is familiar, becomes, in the instance before us, an uncertain phenomenon, to be discovered with difficulty, because the normal structure itself is so delicate that it seems scarcely possible to decide whether the appearance, in any particular case under examination, is really a morbid condition or not.

I have said that analogy would lead us to expect that occasionally, at least, these fibres should exhibit the condition of diminished vital energy, or paralysis. To Dr. Stokes the profession is indebted for having, many years ago, enunciated a pathological law, as regards muscular structure, which has been universally recognized since as a well-established principle. The principle is this, that when muscular fibres have been long exposed to a continued irritation from the existence of inflammation in an adjacent structure, those fibres which at first are thrown into a state of unnatural contraction, by contact with the inflamed parts, at last become paralyzed from over-stimulation. It was in this way he explained the bulging out of the intercostal muscles, which is universally observed in the advanced stage of empyema. But this principle, which is universally admitted, when applied to muscular fibres in contact with serous membranes, is equally applicable to muscular fibres in contact with mucous membranes.¹ In fact, it has been so applied to explain appearances discovered in the dead bodies of persons who have died of ileus. Now, if this be true, and there seems no reason to doubt its accuracy, it fol-

¹ Dr. Stokes has not omitted to notice this fact in his *Treatise on Diseases of the Chest*. He applies it to explain the production of dilatation of the bronchial tubes, and he also says it may be connected with certain forms of dyspnoea, but he has not given any rules for the recognition of the particular forms in which this paralytic condition exists.

lows, as a matter of course, that long-continued inflammatory action in the bronchial mucous membrane ought frequently to impair or even destroy altogether the contractility of the muscular fibres which lie in close contact with the under surface of the inflamed bronchi.

This idea was strongly impressed upon my mind by a case which I had recently under my care in the Adelaide Hospital, which presented some features of a peculiar character, that appeared to me to tally with what might be expected to follow from impaired energy of these fibres, and which derived immediate and obvious benefit from a plan of treatment adapted to such an hypothesis. Under the impression that this idea may be well founded, I venture to lay the particulars before the Association, in the hope that more extended observations may confirm the conclusions at which I have arrived, or, at all events, that the hints which I now throw out may lead to some practical improvements in the treatment of particular forms of chronic bronchitis.

Eliza Simpson, a married woman, forty years of age, the mother of nine children, was admitted into the Adelaide Hospital, December 12, 1859, laboring under a hard cough, with frothy and scanty expectoration, and suffering from intense dyspnœa. Her history was this: She was a laundress by occupation; twelve months previous to her admission, when in a state of perspiration, she had gone out of her washing-room into the open air, to gather in some clothes, when the weather was very severe and snowing. In this way she got cold, which affected her breathing, and from which she had never recovered; within the last four or five weeks her condition had got very much worse, her sufferings being greatest at night; for several weeks she had been totally unable to lie down.

At the period of her admission she was a picture of suffering; she was worn out from want of rest; looked pale and emaciated; her lips were livid, and her respiration, though only 18 in a minute, was forcible and labored. On examining the chest, it was of a rounded form, hyper-resonant on percussion, with a feeling of increased resistance under the pleximeter, and with loud, sonorous and cooing râles, audible in every direction. There was no lateral or antero-posterior enlargement of the thorax during respiration; it moved, as a whole, as if the ribs and cartilages formed one firmly united and unalterable piece, and the only increase its capacity admitted of was by the elevation of its upper margin, and the depression of the diaphragm.

The manner in which the respiration was carried on suffi-

ciently accounted for the pain and difficulty she felt at night. The involuntary muscles not being able, without assistance, to maintain the necessary expansion of the chest to carry on respiration, each act of respiration was, to some extent at least, an effort of volition; and, as the condition of sleep implies the abeyance of sensation, she was obliged to remain awake, to keep herself alive. But there was one feature in her dyspnoea which could not fail to strike the most ordinary observer, and which appeared to me to give it a peculiar character, and that was the great disproportion which existed between the duration of inspiration, and that of expiration. In health, so far as I have been able to observe, the act of inspiration is really longer than that of expiration; at all events, I am satisfied that it does exceed in duration the period of time occupied by expiration. But, in Simpson's case, this ratio was reversed, the duration of expiration being fully three times as long as that of inspiration. This could be seen by the eye, as well as felt by the hand, and heard by the stethoscope. I am well aware, that in chronic bronchitis, a prolonged expiratory murmur, attended by sonorous and sibilous râles, is one of the commonest and best known signs of the disease. But even in cases where the stethoscope reveals this sign, it is rare to see the difference manifested in a marked manner to the other senses, as it was in this instance.

How is this prolonged effort of expiration to be accounted for? I believe the usual way of explaining it is, by referring it to the diminished force with which the air is expelled from the lungs, as contrasted with that with which it enters; hence, when any mechanical impediments exist to the free exit of the air, owing to congestion of the mucous membrane, or the existence of the products of inflammation on its surface, it must take a longer time than usual for the accomplishment of the effort. But, if this were true, how does it happen that such a phenomenon is never observed in acute bronchitis, where such congestion undoubtedly occurs, as well as in the chronic affection? Rejecting for this reason the explanation just given, I attribute the prolonged expiratory murmur to impaired action of the muscular fibres. So long as they retain their healthy vigor and contractility, weak as they are, they materially assist the act of expiration, by simultaneously diminishing the volume of the lungs; but when their vital power is impaired, by reason of the long-continued irritation to which proximity to an inflamed mucous membrane subjects them, they no longer assist, or assist but very feebly and imperfectly, the other agents, whose office it is to diminish the capacity of the chest.

This, then, is what I would consider as the distinctive sign

of paralysis, more or less perfect, of the muscular fibres of the bronchial tubes.

But to return to our case. She was treated on her admission with stimulating expectorants, and turpentine stupes at night, but with very little relief. A variety of expectorants were tried in succession, but without advantage.

On the 26th, the galvanic coil was used, and a current of electricity directed through the chest from the side of the neck to the pit of the stomach. This gave her no annoyance, but was productive of no benefit. On the 27th, the report was: "Looks a little better, but there is no material change; sleeps very little (all her sleep had to be taken in the sitting posture, or sleeping in bed with her head raised up); her breathing is very difficult." On that day she was ordered pills, containing half a grain of extract of *nux vomica* and one grain of *ipecacuanha*, one to be taken three times a day, and her nitric acid mixture to be continued. The effect of this treatment was most surprising, and, I must confess, most unexpected. That night she was able to lie down and sleep all night; her breathing was much easier, and her expectoration free. She looked cheerful, and her appetite, which had been very deficient during the whole period of her illness, was improved. That was the first night, as she stated, that she had been able for seven weeks to lie down and sleep all night. Instead of broken snatches of rest in an uneasy posture, she was able, without inconvenience, to resume the natural incumbent posture, and to enjoy the refreshment that none can appreciate so well as those who have been long deprived of it. This treatment was continued with the happiest effects during the remainder of her stay in the hospital, with the exception that, on the 31st of December, the quantity of the extract of *nux vomica* was increased from half a grain to a grain for a dose—not because the former quantity had in a measure lost its effect, but under the idea that it might the sooner enable her to return to her employment. The effect of the pills, as she described them herself, was to relieve her breathing, promote expectoration, increase her appetite, and keep her bowels open. I certainly was not prepared to find *nux vomica* a promoter of sleep, believing, as I do, that it is in many of its properties an antagonist to medicines of the narcotic class. Yet so it was: the poor creature, who before was almost unable to close an eye, began to sleep in the day-time, to her own infinite relief. It is scarcely necessary to say, that I do not regard this result as contradictory in any measure to the physiological properties of *nux vomica*. It was the consequence, not of its direct, but of its indirect action. It did

not narcotize the sensorium, but it stimulated the muscular apparatus of the pulmonary system, and, by so doing, removed a mechanical impediment to the proper performance of the respiratory functions.

[The Fathergillian prize essay upon the Anatomy of the Human Lung, by A. T. Houghton Waters, will materially assist further investigation on this interesting and obscure subject.—EDITORS.]

III. *Case of Ascites, in which Nature effected a Cure by spontaneous Rupture of the Abdominal Parietes, at a point midway between the Umbilicus and Pubes; with Remarks on the Treatment of Ascites.* By HUGH CROSKERY, L. R. C. S. I., Honorary Member of the Surgical Society of Ireland, Member of the Obstetrical Society, late Assistant Surgeon R. N., &c. (From the Dublin Quarterly Journal.)

I WAS summoned, a short time ago, to see a middle-aged negro, living on the summit of a hill in the heart of the mountains of Jamaica, in whose person nature was said to have effected, and skillfully too, an operation which the annals of medicine tell us she very rarely undertakes, and which it is now my good fortune, as well as my duty, to bring to the notice of the profession.

A case somewhat similar has been described by M. Delmas,¹ in which nature had effected a cure by spontaneous rupture of a pouch which had presented itself at the umbilicus.*

William Jamison, aged 50, had been suffering from ascites for some weeks. His abdomen had acquired a considerable size; the respiration was labored and difficult; the feet, legs, thighs, and scrotum were almost bursting with œdema; he was restless, wakeful and debilitated, and for the last few days a painful tumor, over the centre of the hypogastrium, added to his sufferings. On the centre of this hard and painful spot, a small slough appeared—the poor fellow lay awake nursing his tender burden—when suddenly he felt something give way, his bed was saturated—he was being tapped by nature! The

¹ Dict. de Med., vol. iv., p. 202.

* Dr. Chatard, of Baltimore, informs us that he has also seen a case of spontaneous rupture of ascites at the umbilicus.—*Editors.*

slough had been dislodged, and the fluid was spinning from a small aperture, which now occupied its place! In less than an hour the poor man was completely relieved, and the fluid evacuated. Strange to say, nature had selected the very spot where the operation is performed by art! I saw this man a fortnight after the occurrence, and he told me that the fluid had continued to trickle from the wound for about twelve days, and that, by making pressure with his hands above and below the aperture, he had been able, at any time, to empty out the contents of the peritoneal cavity, without causing himself the slightest pain.

On the day of my visit the aperture had closed, and there was considerable induration of the abdominal wall in its immediate neighborhood. Some of the contents of the abdomen appeared also to have become agglutinated to its edges. The man's general health was improved, the œdema in his lower extremities had subsided, and there was no appearance of any re-accumulation of fluid in his abdomen. In short, the man was cured of his disease, and, from all that I can learn, continues so up to this day.

In Jamaica, the black and colored people of the poorer class are particularly subject to ascites. I have now been three years in the country, and during that time I have had many cases under my care, and the result of the treatment that I have adopted has been very satisfactory. The disease is amenable to treatment, even in its advanced stages, as will be seen by the sequel. The intermittent and remittent fevers of the country, for the production of which this "isle of springs" is famous, are the most frequent causes of ascites, which runs generally a rapid course in constitutions enfeebled by disease, and the ingestion of watery, unnutritious food. In the fruit season, the mango and the orange form the chief diet of the negro, and what is there in these to strengthen an enfeebled frame? A few paroxysms of intermittent fever, or ague, are succeeded by tumefied spleen; this, in its turn, pressing on some of the deeper vessels, impedes the return of blood from the vessels lining the intestinal canal, or perhaps by its size produces unusual irritation; and the natural result is, that in a flabby and relaxed frame, a rapid accumulation of fluid soon takes place.

In the mountain districts the surgeon is seldom sent for until the disease is in a very advanced stage, and the patients require immediate relief. The disease generally assumes the acute or subacute form, and, in the late stages, is seldom unaccompanied by considerable œdema of the private parts and lower extremities. In the negro it is seldom complicated with serious

organic changes of any of the solid abdominal viscera, so that the only difficulty to contend against in the treatment is the restoration of the power of absorption, and the diminution of the function of exhalation, in systems relaxed by previous disease, and impoverished by a poor and unwholesome diet.

In the treatment of such cases, there is almost always a necessity for immediate tapping, for reasons before alluded to; but, apart from this, I make it a rule to draw off the fluid, whether there is immediate necessity for its withdrawal or not, as I always find that if the operation is deferred time is lost, and the trocar has to be had recourse to in the end. After the withdrawal of the fluid, and consequently the pressure exerted by it, the exhaling glands, which before were impeded in their functions, are now easily acted on by remedies administered, and a decided effect is soon produced.

The treatment which I am in the habit of adopting in such cases is as follows:

Take of elaterium, 1 grain; croton oil, 6 drops; blue pill; compound squill pill; powdered aloes and jalap, of each 30 grains; make into 24 pills, one to be taken morning and evening. Take of water of acetate of ammonia, 2 oz.; compound spirit of nitrous ether and juniper, of each $1\frac{1}{2}$ oz.; tincture of hyoscyamus, 2 drachms; tincture of opium and digitalis, of each 1 drachm; nitrate of potash, 2 drachms; tartar emetic, 1 grain; camphor mixture sufficient to make 12 oz. Mix. A large spoonful to be taken every third hour.

This treatment, if persevered in,—pressure being kept up over the abdomen, at the same time, by means of a carefully applied swathe, and the system supported by nutritious food, and braced by tonics,—almost always succeeds with me in preventing a recurrence of the disease. In proof of this, I subjoin a few out of many successful cases.

CASE I.—Wm. Anderson, quadroon, aged 40. Ascites with extensive anasarca. Had been, for some time previous, subject to repeated attacks of intermittent fever. The areola tissue of the whole body was extensively infiltrated, and had become so within a few days; the respiration was painfully difficult, and relief by tapping was urgently required. The man was tapped on the 18th of October, 1857, and eleven imperial quarts of fluid taken away: the latter was highly albuminoid. Pressure over the abdomen was kept up for three weeks by means of a carefully applied bandage, and during that time the treatment, previously described, was adopted. The anasarca soon subsided, and any fluid that may have re-accumulated was soon absorbed. The man made a good recovery, and was soon able to return to his trade, which was that of a coppersmith.

CASE II.—Eliza Harris, a black woman, aged 29. Ascites. Had previously suffered from intermittent fever. Tapped on the 27th of January, 1858; nine quarts of highly albuminoid fluid drawn off. The same treatment was adopted as in the preceding case, and with the same successful result.

CASE III.—Mary Rodney, a black woman, aged 50. Ascites. Tapped on the 10th of February, 1859; nine quarts of fluid removed. Recovery after same treatment.

The short synopsis of the preceding cases is quite sufficient for all practical purposes, and it would be only a waste of time and space, either to give the minute details of each case, or to enumerate other successful ones. Ascites is so common a disease among the negroes, that one seldom thinks of noting down any case except something of unusual interest presents itself. It must not, however, be imagined that all cases are so easily managed as those enumerated in this communication. Cases sometimes present themselves, in which serious organic disease has been the cause of the mischief. I published in the February and August numbers of the "Dublin Hospital Gazette," for 1858, an interesting case of ascites, the result of a chronically enlarged spleen, where, after two tapplings, and after everything else that could be thought of had been tried, the disease was completely cured by mercury. The subject of the case referred to is now a strong, healthy boy, and although the spleen is still about four times its natural size, it exerts no baneful influence.

IV. *Diminished Capacity of the Pelvis considered with respect to the Expediency of Marriage.* By Mr. PAJOT, Prof. pro tem. Hospital of the School of Medicine, Paris.

THE capacity of the pelvis may be reduced below its natural standard, although its relative proportions are preserved; more frequently, on the contrary, it is not only too narrow, but is also deformed, a condition which, according to Mr. Pajot, more especially requires consideration. These coarctations attended with deformity give rise to questions which the practitioner is called upon to answer, and in which the opinion expressed may involve a considerable amount of responsibility.

In the case, for instance, of a more or less deformed girl, for whom a desirable matrimonial engagement offers, the mother

will naturally consult her physician, and probably states that, being about to marry her child, and feeling some anxiety on account of her shape, she is anxious to know if her daughter can become a mother without danger. The question is obviously one of a very delicate nature; for should the physician reply in the negative, he condemns the girl to celibacy, and if in the affirmative, and labor at a future day be attended with difficulties, they may be made the occasion of bitter reproach. Under these circumstances, said Mr. Pajot, the practitioner should take a general view of the individual, so as to form some opinion, from general appearances, of the shape of the pelvis. Women with a contracted pelvis are usually small, and their countenance offers a highly suggestive, though undefinable, peculiarity of expression. After noting the result of this first superficial investigation, the girl should be requested to walk, and her gait and figure may afford further indications. A waddling motion will, perhaps, be detected, or a slight lameness, or a raising of the back of the dress by unusual motion of the limbs. Certain women have very low backs, with high shoulders; but there are innumerable shades of difference in the attitude, as many, perhaps, as individuals. It should further be remarked whether the head is large and the fingers short. These surface notions acquired, the precedents of the case and the early history of the subject must be inquired into. It will generally be stated that the girl has been affected with rickets, or that she met with some accident, such as fracture of the thigh, or dislocation of one or both hips; spontaneous dislocation may be stated to have taken place at the age of three or four, in consequence of inflammation of the hip-joint. This information gives the physician a clue to the truth; a coarctation is probable, but must not be asserted yet to exist, as error is easily fallen into. Mr. Dubois relates a singular case of malpractice arising from foregone conclusions: A Parisian practitioner, who was summoned to attend in labor a lady afflicted with an enormous deviation of the spine, concluded from the existence of this deformity that the capacity of the pelvis was much diminished. The head presented, and he attempted to turn the child, failed, and finding himself unable either to extricate the foetus, or alter its position, perforated the cranium. Now the pelvis was in a perfectly natural condition, and if the accoucheur had displayed common patience, instead of prematurely interfering on the grounds of mere suspicion, the child would have been born naturally. The proportional length of the lower extremities must also be estimated. This will supply an important item to the solution of the problem; whether the vertebral column be short or long,

straight or deviated, if the legs are more or less bent, the femurs much incurvated, should the legs, and more especially the thighs be short, very short, like those of ricketty children, be careful, there are high probabilities that the pelvis is vitiated. On the contrary, whatever be the condition of the spine, if the lower extremities are straight, and long in proportion to the bust, it is likely that the pelvis is regular.

This presumption is, however, not a certainty; but it is not on mere suspicion that marriage should be prevented.

Mr. Pajot then enumerated the numerous instruments contrived for the mathematical measurement of the pelvis, and showed the difficulties which attend the investigation. The defect of these instruments lies in the impossibility of using them with desirable precision. Baudeloque's pelvimeter, which has the advantage of being used externally, affords only a means of comparison, a statement easy of demonstration.

The external sacro-pubic diameter of a well-formed pelvis should reach 19 centimètres ($7\frac{3}{5}$ inches), viz:

$6\frac{1}{2}$ centimètres (2 inch. 7 lines) for the thickness of the sacrum;

$1\frac{1}{2}$ centimètres ($7\frac{1}{2}$ lines) for the depth of the pubes;

11 centimètres (4 inch. 5 lines) remain, which represent the inner sacro-pubic diameter, the proper dimension of the conjugate diameter of a healthy pelvis. Suppose it to be contracted and that the outer measurement does not exceed for instance 6 inch. 5 lines, deduct 3 inches and $\frac{1}{4}$ for the combined depth of the sacrum and symphysis pubis, a very little over 3 inches will remain, a space insufficient for the accomplishment of natural labor. This estimation, said Mr. Pajot, reposes, however, on the conjecture that the depth of the sacrum and symphysis is invariably the same. Now, six years ago, this gentleman met with a sacrum which did not exceed one-half of the usual thickness; hence it follows that, if the thickness of the bone is liable to variation, Baudeloque's pelvimeter becomes useless. Mr. Van Huevel has been struck by this fact, and after measuring the pelvis externally, he computes separately and successively the depth of the sacrum and that of the symphysis. This may, in theory, be sufficient, but it is far from satisfactory in practice. The promontory of the sacrum is reached readily, but it is extremely difficult, if not absolutely impossible, to secure its contact with the rod of the instrument.

Under these embarrassing circumstances the following course should be adopted: The precedents of the individual having been investigated, her figure and gait having been observed, and the limbs and outer pelvis inspected, the contents of the

intestine having previously been removed with an enema, she should be placed reclining on a couch, the arms hanging, in the attitude requisite for exploration of the womb; the surgeon then, resting his elbow firmly on the bed, very gently passes the finger into the vagina, and slowly ascends to the promontory of the sacrum, which he meets the more easily, in proportion to the diminished capacity of the pelvis. If with the finger the promontory cannot be reached, the pelvis may safely be asserted to be free not from all deformity, but from any considerable degree of contraction. Should the explorer, on the contrary, be able to feel the promontory with the extremity of the finger, the index of the other hand should be placed behind the meatus to estimate the depth of the pubic arch, and a mark then made with the nail upon the finger previously inserted, at the spot corresponding to the inner ridge of the symphysis; and by deducting five lines from the measured distance, on account of the obliquity of the mensuration below the pubic arch, the length of the antero-posterior diameter will very closely be computed.

This, in Mr. Pajot's opinion, is the best mode of solving the question; it affords, it is true, no clue to oblique or transversal deformations, which are very difficult to estimate, but fortunately it is not absolutely necessary to do so. The most important point is to acquire a correct knowledge of the antero-posterior diameter, and the procedure above described supplies the means of obtaining this measurement with almost mathematical precision.

V. *Lectures on Experimental Pathology and Operative Physiology*, delivered at the College of France, during the winter session 1859-60. By M. CLAUDE BERNARD, Member of the French Institute, Professor of General Physiology at the Faculty of Sciences.

ON EFFECTS OF WOORARA.

GENTLEMEN,—In the eyes of the physiologist and medical philosopher the effects of poisons are, as we have recently proved, exactly similar to those of disease. You will not, therefore, be astonished to hear that a wide difference exists between the various degrees of intensity with which poisons act upon the animal frame under given conditions; a circumstance which

clinical observation easily enables us to foresee, by pointing out the greater or lesser liability of individual patients to peculiar diseases; old age, for example, is the favorite field of certain affections, and childhood that of others; and it even appears that some few disorders are exclusively confined to a limited period of life.

A similar predilection (so to speak) is exhibited by toxic agents for animals placed under particular circumstances: the young are sometimes destroyed by poisons which exert no influence whatever upon the adult; at other times the reverse takes place; the first stage of life remains uninjured, while fatal effects are produced in animals which have already attained their full development. Woorara, the substance which must occupy our attention to-day, stands prominently forth among this latter class of poisons. The fearful effects to which this agent gives birth in full-grown animals are never observed during the embryotic state, and within a few moments the experimental proof of this assertion will be placed before you; but previously to doing this, we think it proper to furnish you with an explanation of the fact.

When the tissues of animals destroyed by woorara are examined, it is easily ascertained that the motor nerves have been deprived of their physiological properties, and that having lost the power of producing muscular contraction under the influence of the will, they remain equally insensible to the action of nervine stimulants. All the other tissues of the body being found uninjured, it is plain that asphyxia has been the cause of death, the respiratory motions being altogether paralyzed; and in consequence, when artificial respiration is had recourse to, life is kept up for a sufficient space of time to allow the poison to be expelled; when this has been accomplished the animal is out of danger.

But in administering this substance in small quantities the intensity of its effects may be reduced to the very lowest degree, although their nature remains identically the same. The respiratory motions may thus be slackened without being altogether suspended, when woorara is instilled, drop by drop, into the vessels; and in this manner the animal may be reduced to five or six inspirations per minute—a quantity just sufficient to support life. Let us now suppose for an instant the process of respiration to be entirely independent of muscular action in a given case; it is clear that woorara could not, under such circumstances, paralyze any of the functions indispensable to life. Now such are precisely the conditions in which the embryo is situated; it breathes by means of a temporary apparatus, and the revivification of the blood is absolutely independent of mus-

cular power. Such is the rational explanation of this singular fact, which fully confirms our former ideas with respect to the part played by the motor nerves in all cases of poisoning by this substance.

The preceding reflections afford an ample demonstration of the fact that the destructive powers of woorara are proportioned to the physiological activity of the respiratory functions in each given case. Of all living beings birds are those which most speedily experience its fatal effects; mammals come next; reptiles and fishes occupy a much lower place in the scale; and last of all we find the embryo, which as it breathes in a totally different manner from the adult animal, escapes altogether unharmed from the action of this formidable agent. The result is, that on injecting woorara into the veins of a pregnant female the mother is destroyed, while the foetus survives, and only dies after a certain space of time, in consequence of the total cessation of the parental circulation.

You are aware that young fishes exhibit for some weeks after birth a very singular appearance; an enormous tumor occupies the ventral region, and offers a far more considerable size than the small streak which lies on its upper surface, and represents the principal part of the animal's body. Now, this voluminous globe is merely the umbilical vesicle, which has not yet been absorbed; as long as this change has not taken place, the little animal neither feeds nor breathes in the ordinary sense of the word; its gills are motionless, but the numerous vessels which ramify over the surface of the umbilical vesicle absorb oxygen by a direct process, without any motion taking place. Little fishes, therefore, remain in the foetal state long after birth, with reference to the respiratory process; and we are happy to seize upon so favorable an opportunity to show that on animals organized in this manner the action of woorara is altogether powerless. In the basin which we now place before you, several little fishes, in the state we have just described, are freely swimming in an aqueous solution of woorara, and do not appear to suffer the slightest inconvenience from the presence of this agent. The case is widely different with respect to the gudgeon contained in this other vessel; you see that the animal is already dead; for in adult fishes the respiratory process is connected with the gills, which are set in motion by a special muscular apparatus. In this case, the poison has been absorbed by their internal surface, the animal having been plunged into the liquid without the slightest incision being made to allow the deleterious substance a direct passage. The same phenomenon takes place in mammals, when woorara is directly brought into contact with the inner surface of the pulmonary apparatus.

In order to render the contrast still more striking, if possible, we have placed some other little fishes in a solution of upas antiar; and death has been, as you perceive, speedily produced. How is this difference to be explained? The action of upas, instead of paralyzing the motor nerves, destroys the properties of the muscular fibre itself, and arrests, in the first place, the contractions of the heart,—a cause of destruction from which the embryo itself cannot of course escape.

Nor is this all. Our little fishes, as you may see, rapidly swim about in the liquid impregnated with woorara, and freely make use of their muscles, which this poison has not been able to paralyze. We meet here with a second difficulty; how can we understand the prodigious difference in the effects produced in the embryo and in adult animals? The reason is, that during the foetal state the muscular apparatus enjoys an independent power of contraction, and does not obey the motor nerves. Take a hen's egg during the latter part of the process of incubation; break the shell, and place it on a warm stove; the heat provokes distinct motions in the limbs of the young animal, which offer some analogy with those of the foetus *in utero*; yet, if the nerves are laid bare and then galvanized, no contraction whatever is the result. The fact is, that in the foetal state the separate existence of the various systems is the rule; while the reverse takes place in the adult being. The embryo's heart beats long before nervous influence is brought to bear upon its contractions; and its muscles are capable of acting long before they have assumed the fibrillary state, and while merely composed of a series of cells. There exists, in this respect, a striking analogy between the embryo and animals belonging to the lower degrees of the scale, in which the nervous system either does not exist, or exerts no visible influence on voluntary motions; thus, when leeches are poisoned with woorara, they remain motionless for a time, when left undisturbed; but when stimulated, immediately set forwards; after a few hours, the animal resumes its pristine activity. In polypi, the action of this substance altogether disappears; for in such beings there exists no nervous system for the poison to work upon.

The action of woorara upon the motor nerves is, therefore, a positive fact, which now lies beyond the reach of all objections; and by observing the results which follow its introduction into the system, each animal's position in the scale of life may in some manner be ascertained. In order to convince you of this, we shall simultaneously poison a frog and rabbit before you; the influence of woorara will in the first case be slow to exhibit its effects; while, in the second, it will almost instantaneously be felt.

[The experiment is performed. A few drops of a strong solution of woorara are introduced under the skin of a rabbit. A large and vivacious frog is simultaneously poisoned with a proportionate quantity of the same substance. During the first three minutes the rabbit does not appear to feel the slightest inconvenience, and when set at liberty jumps down from the table and endeavors to make its escape. At the expiration of three minutes and a half it grows weak, and can hardly stand on its legs; after five minutes, the animal is completely motionless, and respiration no longer takes place; the heart does not, however, cease to beat before ten minutes have elapsed. The phenomena produced in the frog are identical, but death does not take place before seventeen minutes.]

We shall now, gentlemen, examine in succession the various tissues of these two animals, and you will find that the nerves alone have been affected. We shall then consider the results of this lesion, and you will perceive that scientific analysis, even when carried as far as possible, frequently remains powerless to explain the phenomena which daily meet our eyes.

Since the period when my labors on this subject were first placed before the public, (in 1850,) the question has attracted the notice of several distinguished physiologists; it is directly connected, in fact, with one of the most important points in the science of life, viz., the respective independence of the muscular and nervous elements. There exists in all muscles a contractile property, which Haller described under the name of irritability. This power obeys the nervous impulse in the normal state, and yet enjoys a separate existence. The movements of the embryo are a proof of this; and the action of woorara, after absolutely destroying the stimulating property of motor nerves, does not prevent the muscles from acting as usual under a direct stimulus. Before these experiments were known, it was contended that in galvanizing the muscle itself, the minute nervous filaments disseminated in the contractile tissue were excited, and in this manner alone contraction was produced; but the effects of woorara, and other poisons, enable us to lay down an absolute distinction between the properties of the two systems concerned in locomotion.

We take a healthy frog out of this jar. The skin being removed, we lay bare the sciatic nerve, and galvanize it; convulsions are instantly produced in the corresponding posterior limb. The experiment being repeated on the frog we have just poisoned, exhibits the complete indifference of the muscles to excitations brought to bear upon the nervous system. Where is the difference between these two sets of nerves, the physiological properties of which are so completely opposed? Let

the paralyzed nerve be placed under the microscope ; no alteration appears in its intimate structure. Let its chemical composition, and physical properties be in their turn examined ; no modification whatever is found to have taken place. We cannot foresee the ultimate results which science may possibly attain to some day, and the efforts of anatomists will, perhaps, at some future period be rewarded with the discovery of the precise alteration produced in the nerves by the effects of woorara ; but up to the present moment, no satisfactory solution of the difficulty has been propounded.

Professor Jacobowich, whose remarkable labors on the structure of the nervous centres are perfectly well known to you, has deeply studied the question, as far as microscopical anatomy is concerned, in animals killed with woorara, prussic acid, and other toxic agents, which exert an immediate influence on the nervous system ; and he states that in all such cases, he has met with ruptured nerve cells, and broken cylinders, in the spinal cord, concurrently with other lesions of the same kind. It has also been supposed that woorara is chemically combined with the nervous substance, and acts upon it in the same manner as oxyde of carbon on the blood-globules ; but neither of these ingenious explanations appears adapted to the facts of the case, when it is borne in mind that artificial respiration, kept up for a space of time, enables the animal to recover. How could a similar result be obtained if the nervous system had undergone a complete disorganization ; or if a chemical compound of a peculiar nature, but no longer apt to transmit the nervous fluid, had been the result of the experiment ? The nervous system in this case would never be able to recover its physiological properties.

Others have expressed the opinion that the nerves themselves are not the real seat of the injury produced, but that a peculiar tissue which serves as a medium between the muscular and nervous elements has been affected by poison, but the same objections as before are applicable to these views ; if a permanent change has been produced, how is it that the effects of the poison vanish as soon as the noxious substance has been eliminated from the system ? Besides, if the nerves are no longer capable of transmitting the voluntary impulse, the fact is sufficiently explained by the disorganization of their ultimate extremities, without having recourse to the destruction of an intermediary tissue, the existence of which is far from being established.

We are indebted to M. Betzold for some beautiful experiments upon this subject. The rapidity with which the nervous system transmits the impulsions which it has received has been

calculated by Professor Helmholtz; he ascertained that it oscillates between sixty and eighty metres per second (190 to 240 feet.) When the nerve is of considerable length, as in the case of the sciatic trunks, a sensible difference is perceived in the swiftness with which contractions are produced, when the stimulus is applied to its root, or its distant extremities. The normal rapidity of the phenomenon having been carefully ascertained in a sound animal, M. Betzold slowly poisons it with a weak solution of woorara, and carefully notes the progressive diminution which succeeds in the swiftness with which the nervous fluid is transmitted to a distant point; it falls from sixty metres per second (the absolute minimum in the healthy state) to five metres: below this point, the transmissive property disappears altogether.

But however deep the perturbation occasioned in the motor nerve, with respect to its physiological activity, all its physical properties remain intact; the electric currents described by M. Dubois Raymond still continue to exist, and retain the same degree of intensity of action. The same is the case as far as other properties of a merely physical order are concerned.

In conclusion, it is evident that the physical, chemical, and physiological characteristics of the tissues which compose the animal organization, are entirely distinct, and exist independently of each other; and, in this particular case, the action of woorara would appear to be strictly confined to the physiological properties of the nervous system; for, in every other respect, the injured nerve is perfectly identical to the sound one. There remains, therefore, an extensive field for future investigations; and the discovery of the secret still withheld from us cannot fail to interest the working physiologist in the highest degree; for a strong light will, in this manner, be thrown on the most intimate properties of the nervous system.

CHRONICLE OF MEDICAL SCIENCE.

PHYSIOLOGY.

1. *On the Physiology of Digestion.*

Prof. Busch, of Bonn, has had the opportunity of making experiments on digestion upon a woman who had been tossed by a bull, and presented, in consequence of the accident, a fistulous opening communicating with the small intestines. The fistula was so complete that the bowel was divided into two perfectly distinct halves. The upper portion consisted of the stomach, the duodenum, and of a probably minute piece of small intestine; the lower portion was composed of the remaining part of the small intestine, the colon, and rectum. Through the upper half, the food introduced into the stomach, as well as the digestive fluids of the latter organ, the liver and the pancreas, escaped, no part of them finding their way into the lower half. This state of things was therefore favorable to the study of the action of the stomach, of the biliary and pancreatic secretions and also of intestinal secretions independently of the liquids just named.

One of the first effects of the pathological state of this woman was a considerable loss of flesh, as observed when she came into the hospital, six weeks after the accident. Her appetite was, however, insatiable, though she was as weak as those animals in whom artificial fistulæ are made. She was also very drowsy and cold; but this temperature was merely objective, for a thermometer introduced into the intestine marked a normal heat. All these symptoms disappeared when the patient recovered a little strength, in consequence of a generous diet.

She used to swallow an enormous quantity of food without feeling satisfied; but by thus eating largely she felt better, though still hungry. When the stomach was empty she felt ill. The woman was so thin that the coils of intestines could be seen through the parietes of the abdomen; and it was observed that their peristaltic movements were as energetic as

those of that portion of the intestine situated above the fistula and open to view.

As the intestinal secretion or juice was perfectly pure and unmixed with any chyme, which latter all escaped by the fistula, a good opportunity was offered for studying the nature of that juice. Prof. Busch found the quantity always small, and tried its effects upon protein compounds, starch and cane-sugar, these being the first experiments of the kind ever made. The patient was at the same time fed by the introduction into the lower part of the intestine, through the fistula, of beef-tea, beer, soups with flour, meat, hard-boiled eggs, &c. Soon after these injections were resorted to, she had numerous stools, a circumstance which had not been observed since the accident. The evacuations had a well-marked smell of putrefaction, without any undigested portions of meat or hard-boiled eggs being noticed in them; this being a clear proof that the intestinal juice acted as a solvent upon the food passing through the canal.

Mr. Busch used to wrap the various substances introduced in a piece of muslin, after having carefully weighed them, in order to observe the action of the intestinal juice. He noticed that it was principally upon starch that this juice exerted an energetic solvent power.

An interesting point was to find out what would become of fatty matter without the assistance of bile or pancreatic juice. According to expectation, fatty substances passed without being absorbed, or at least but a very small portion of them disappeared.

M. Busch also examined the state of the substances which escaped by the upper portion, namely, those which had been subjected to the action of saliva, the gastric juice, bile and the pancreatic juice. A very extraordinary fact observed was, the rapidity with which the alimentary substances escaped. In from fifteen to thirty minutes after the ingestion of the food by the mouth, it was observed to escape by the fistula; hard-boiled eggs appeared in from twenty to twenty-six and thirty-five minutes; cabbage took from fifteen to nineteen minutes; meat from twenty-two to thirty minutes; potatoes fifteen minutes. When the meal was plentiful, complete digestion required from three to four minutes(?).

The substances which escaped by the upper end of the divided canal seemed at first sight to have undergone but little change; they were, however, considerably softened, and the meat presented both longitudinal and transverse cracks or slits. M. Busch thinks that the fluid in which these substances were suspended contained no longer any saliva.

We add a few of the propositions which the author considers as proved by the experiments above enumerated :

1. The peristaltic movements of the intestines are as vigorous when the bowels are covered by skin as when they are exposed to the air; they withstand the pressure of a column of water two feet high.

2. The intestinal tube has periods of rest and motion.

3. The intestinal juice is secreted in small quantity; its re-action is always alkaline; and it contains, on an average, 5.47 per cent. of solid matter.

4. It decomposes starch and protein compounds.

5. It changes starch into grape sugar.

6. It decomposes protein compounds with the phenomena of putrefaction.

7. It does not change cane-sugar into grape-sugar.

8. Cane-sugar, when wholly absorbed, does not re-appear in the urine.

9. Fat which has not been brought in contact with the bile or pancreatic juice, is either not absorbed, or, if so, in very small quantities.

10. The first portions of the food introduced into the stomach reach the first third of the small intestine, on an average, in from fifteen to thirty minutes.

11. Cane-sugar held in solution disappears almost entirely at the beginning of the intestinal canal; any such cane-sugar which reaches the small intestine is changed into grape-sugar.

12. Unboiled white-of-egg is absorbed in the stomach, or the first part of the intestine; the portion which goes beyond has not undergone any change.

13. Gum is not changed into sugar; it passes into the intestines without alteration.

14. Gelatine becomes dissolved, and loses the faculty of coagulation.

15. Traces of caseine in solution are found in the intestine after the ingestion of milk.

16. Fat forms an emulsion with the fluids which find their way into the small intestine, when these fluids have an alkaline re-action; the emulsion is incomplete when they are acid.

17. The mixture of juices in the small intestine has a digestive action on the protein compounds.

18. The minimum of the digestive juices, which reach the upper part of the small intestine in twenty-four hours, weighs more than one-seventeenth part of the whole body.—*Archiv. für Path. Heilk. and Gazette Médicale de Paris.*

2. *Influence of the Sun's Rays in the Production of Organic Matter.* By CHARLES T. JACKSON, M. D. (From the Boston Medical and Surgical Journal.)

Plants alone possess the property of converting inorganic and mineral elements into organic substances, a power wholly denied to animals. They are enabled to effect this wonderful conversion through the influence of the solar rays, and chiefly by the decomposition of carbonic acid gas, the carbon being separated and combined with other elements, while the oxygen is given off and goes to form that portion of the atmosphere essential to the respiration of all animals. So rapid is the operation of the foliage of plants in abstracting carbon from carbonic acid of the air, that if we place a green and leafy bough of a tree in a glass globe, and place it in the sunshine, and then blow air through the globe, by means of a pair of bellows, the air, after passing over the foliage, will be found deprived of all its carbonic acid, and oxygen will have taken its place.—(Dumas.)

When we draw a breath of air into our lungs, and then exhale it, though it has been but a moment in the lungs, it will be found so charged with carbonic acid gas, as to extinguish a burning candle. This experiment is easily made by fixing a glass tube, inserted through a cork, into the top of a glass bell or receiver, open at the lower part, and placed in a vessel of water, and drawing the air from the receiver into the lungs, so that the water will rise and fill the vessel, and then breathing back the air into the receiver. Now if the cork is removed, and a lighted candle is lowered into the bell, it will be immediately extinguished. To show that foliage, in sun-light, will restore the respirable properties of the air, bend a leafy bough so that it shall pass under the edge of the bell and come in contact with the vitiated air, and expose the whole for a few minutes to sun-light. The carbonic acid will be decomposed, and the carbon being removed, oxygen will be left in the bell, and the candle being again applied will burn freely.

Water is not decomposed by the respiration of fishes, but only the air, dissolved in the water, goes to support their respiration, the proportion of air dissolved in water being, on the average, about $2\frac{1}{2}$ per cent. of its bulk.

Aquatic plants depend upon the small quantity of carbonic acid that is dissolved in water for the production of their carbonaceous tissues and juices, and they, like other plants, decompose carbonic acid through the influence of the solar rays, and give out oxygen. Thus plants and fishes aid each other,

the one producing the proper respirable food for the other. Without aquatic plants, water would soon be unable to sustain the respiration of fishes; and hence in natural lakes and rivers, aquatic vegetation maintains the water in its proper condition for the respiration of these animals. So in well-balanced aquaria, the proper proportions of animal and vegetable life may be kept up, and, provided there is sufficient sun-light, no mechanical admixture of air is needed; for the plants will reproduce the oxygen from the carbonic acid exhaled from the fishes' gills.

The atmosphere is an ocean of mingled gases, chiefly nitrogen and oxygen, with a small proportion of carbonic acid, the proportions being, nitrogen 76.9, oxygen 23 per cent. by weight, while that of carbonic acid varies from three to sixteen thousandths parts. Aqueous vapor, in variable proportions, is also dissolved in the air, the quantity depending on the temperature. Owing to the law of diffusion of gases, there is no separation of the heavier from the lighter by gravitation, and Gay Lussac found carbonic acid in the air he brought down in his balloon from a height of more than five miles, while Saussure ascertained its existence uniformly in the air over the highest peaks of the Alps. In large cities there is an accumulation of carbonic acid, owing to the want of free circulation of air, and the absence of an adequate amount of foliage for its removal; but it rarely accumulates in sufficient quantities to materially affect animal life. We live, then, at the bottom of a great atmospheric ocean, more than fifty miles deep.

The sun's rays penetrate readily through this atmosphere, and affect plants and animals on the earth's surface. From the dawn of creation—from the time when "God said let there be light, and there was light"—for myriads of ages, has the glorious orb of day been engaged in performing his beneficent work, and long before the creation of man the solar rays were busily employed in the preparation of the world for his advent.

Solar heat, absorbed by the wide-spreading ocean, at a time when scattered islands existed in the place of our present continents, warmed the waters, which so retained the heat as to give an almost tropical character to vegetation, even in climes far removed from the equator; and the small area of land above the surface of the ocean radiated away into space but a small proportion of heat. Hence, perhaps, the much wider extension of the tropical flora in ancient times, when the great coal formations were produced, and the remains of tropical animals in regions too far removed from the equator to allow of their existence in those regions now.

Some geologists think that the equalization of temperature,

by circulating warm water, is adequate to account even for the fossil flora and fauna of Melvill's Island and of the Siberian Coast, and also of the Arctic regions of America. Brogniart supposes that there was originally a larger proportion of carbonic acid in the air than now exists, and that under those favorable conditions of greater warmth and a humid climate of the oceanic islands, a rank vegetation grew and rapidly abstracted carbon from this gas, and converted it into those plants of which coal was formed.

By this operation ages of sunshine became converted into fossilized light and heat; for submerged plants were changed into bituminous coals, which at present supply us with light and heat, both of which, in the form of coal, were stored up long anterior to the creation of man.

This wonderful provision of coal, the source of most of the light and heat we enjoy in our dwellings—this accumulated and almost incalculable source of power concentrated in the bowels of the earth, was prepared by that Being who created man, long before his coming, and thus the world was in the earliest ages fitted for the labors of civilized life, and the arts were provided with their most indispensable first materials, and the source of their greatest power.

MEDICAL PATHOLOGY AND THERAPEUTICS.

1. *On Epidemic Dysentery.* By Professor TROUSSEAU.

The year 1859 was remarkable in France for the prevalence of a terrible epidemic of dysentery. While in former years the affection has been observed only in circumscribed localities, it prevailed during the past year almost universally. Paris, too, which has perhaps been exempted from epidemic dysentery for a century, has had, on the present occasion, to pay a large tribute. Commencing towards the end of July, the epidemic attained its maximum in September, undergoing a notable diminution in intensity towards the end of October. Of all epidemic diseases, dysentery is the most murderous—typhoid fever, cholera, diphtheria, variola, and scarlatina being but as child's play compared with it. These affections prevail only accidentally, while dysentery decimates whole populations, re-

turning at certain fixed epochs, as every three years, for example. Desgenettes declared that it killed more soldiers than the enemy's cannon did between the years 1792 and 1815. The etiological circumstances of the invasion of an epidemic may be quite inappreciable. Thus, at Tours, there are two barracks placed in identical hygienic conditions, and yet, during thirty years, it has always been the cavalry barracks in which dysentery has prevailed epidemically. The reputed effects of the excessive use of fruits in generating the disease is very doubtful, seeing that it sometimes rages when fruits are very scarce, as in 1859, while it may not be met with when they were in excessive abundance, as in 1858.

Passing by M. Trousseau's description of the disease, we come to his account of the treatment. His right to speak with some authority upon this point, is derived from the fact of his having witnessed four epidemics of the disease at Tours, Versailles and Paris, during which the victims were either young and vigorous soldiers, aged men and women, or young children. Moreover, as Reporter on Epidemics to the Academy he has to peruse the accounts of the various epidemics which appear throughout France. Some thirty or forty years since the traditions of the former age were abandoned, Broussais sweeping away the whole of the empirical modes of treatment in favor of his doctrines. In fact, with an inflammation so violent in view, it was then difficult not to give in to them; and the antiphlogistic treatment was put freely into force, and when unsuccessful, this was believed to be because it had not been carried far enough. In 1823 or 1824, however, M. Bretonneau, imbued with the medical doctrines of Stahl and Sydenham, set on foot a re-action against the doctrines of Broussais, by resorting to a substitutive mode of treatment. He gave an ounce of the sulphate of soda internally, and administered the same dose in a very copious enema, once or twice a-day, continuing the practice as long as the stools remain bloody. As soon as they became bilious and serous, the sulphate was only given once a-day, then every other day, and afterwards at still rarer intervals. In 1828 or 1829, M. Trousseau published an account of an epidemic treated with success in this manner. In 1842, an epidemic occurring in the garrison at Versailles was similarly treated, but with less marked success; however, at all events, the military surgeons in attendance—almost all pupils of Broussais—agreed that the sulphate of soda was preferable to blood-letting. Unanimity in favor of neutral salts, of one kind or another, has also nearly prevailed in the reports addressed to the Academy from all parts of France. Frequent failures have undoubtedly occurred, but, in

general, when advice is sought early, considerable and extremely rapid success is the result. Induced by the success of the calomel treatment employed by the English at Gibraltar, M. Trousseau has several times put it into force, and frequently with good effect in severe cases of dysentery, occurring, however, sporadically. He still resorts to it when the weather is very hot, but in cold and wet seasons he has found salivation and other ill consequences result from its employment. In children, too, who can only be got to take the sulphate of soda with the greatest difficulty, he prefers giving calomel. Ipecacuanha, which was so much in vogue during the last century, is now seldom employed. Opium is one of the sovereign resources of the *materia medica*, and is perhaps the pharmaceutical substance with which most harm may be effected. It is in incessant use, and is strangely abused, being, in M. Pidoux's happy phrase, the "knout of the therapist." With it every patient who complains or suffers is fustigated. In vain may you try the rational procedures consecrated by usage, and in vain do you appeal to your intelligence and your experience—all goes for nothing—pain is present, and the indication which dominates all others is to assuage such pain, for which opium must be prescribed. With such logic as this we make but a bad business of it, or may engage in a very perilous work. A distribution of opiates with easy compliance is the mark of an impatient and ignorant practitioner. It is a very convenient procedure, and one to which every capacity is competent; which consists in "drying up the intestinal canal" by laudanum in a case of diarrhoea, and in roughly imposing silence upon the symptom pain in a case of dysentery attended with horrible tormina. "I do not pretend to say that, after having put into force the evacuant treatment, that we must never, when the patient is suffering cruelly, temper his pains by a few drops of laudanum, but I entirely object to the practitioner at once drying up the intestinal canal (for this is the aim) in a case of diarrhoea or dysentery. Let him not meddle with opium except with cautious reserve, or he will be the cause of the typhoid symptoms, which will soon make their appearance." After passing in review the various other means of treatment, to which he does not seem to attach much importance, M. Trousseau adds, that all these means will be of little avail if not adopted prior to the occurrence of important pathological changes. Otherwise, every effort will be paralyzed, and no means will avail against the horrible ravages of an epidemic. In conclusion, above all things, let the condition of the diet be attended to, for this is of vital consequence. Insist that two, three, or even four quantities of soup (*potage*) be taken daily,

and prescribe feculent drinks, as barley and rice waters. In all the comparative trials which have been made of treating dysentery by rigorous abstinence, or by allowing aliment in wise moderation, advantage has attended the latter procedure. *Gazette des Hôp.*, Nos. 10 and 14.

2. *Millstone-Makers' Phthisis—Siliceous Material found in the Lungs.*

In the January number of the Medico-Chirurgical Review for the current year is an important paper by Dr. Peacock, on "French Millstone Makers' Phthisis." It appears that there are in London a certain number of masons whose special avocation it is to prepare for use a peculiar kind of millstone known as the French Burr. This stone consists essentially of flint, and has for centuries been quarried at La Ferté, on the Marne, to the east of Paris. The peculiar hardness of this stone renders it much more dangerous to work, and it is much more liable to chip into small particles of extreme sharpness than are the stones more commonly employing in grinding, such as the Yorkshire or Derbyshire grit, the Scotch granite, or the German basalt. The "picking" of these "burrs" is effected by a steel chisel struck by a hammer, and every stroke is attended by a flash of light and a cloud of dust and small particles of stone.

There are, it seems, only four shops in London where these stones are worked, and Dr. Peacock estimates the whole number of those employed upon it as probably not more than fifty. Having seen many of these men as patients at the Victoria-park Hospital, on account of chest affections brought on by their employment, he was led to make extended inquiries at the places of work, the results of which he embodied in the paper to which I have referred. The subjoined are the principal of his conclusions:

1. That the average age of those engaged in this occupation is very low indeed. Of twenty-three who had been apprenticed to it, the average age was only 24.1 years; and one of the foremen stated that the longest period he had ever known a man to work at it was thirteen years.

2. That the fatality among these men is directly due to their inhalation of particles of silix; but that the injurious influence of the latter is much aggravated when the men are intemperate.

3. That the form of the disease induced may be either chronic bronchitis or phthisis, according to the predispositions of the patient.

4. That the presence of the siliceous particles in the lung-tissue may be proved by chemical examination.

3. *Santonine*. By Dr. FALCK.

Professor Falck, of Marburg, in relation to Dr. Martini's observations, gives an account of a series of experiments upon the action of santonine and "santonine-soda" instituted by himself and Dr. Manns, one of his pupils. The santonine was injected into the veins, or under the skin of animals, or was introduced into the stomach. The following are some of the conclusions which he draws from the consideration of the results of these experiments, and from a review of all the other publications which have taken place upon the subject: 1. Santonine and santonine-soda (prepared by boiling together equal weights of santonine, crystallized carbonate of soda and water,) are two poisons undoubtedly possessed of medicinal qualities. 2. The direct introduction of a solution of santonine in weak spirit in a moderate quantity rapidly induces the death of the animal. 3. The santonine and santonine-soda gain access to the blood when introduced under the cellular tissue, or into the stomach. 4. Under conditions, not as yet well ascertained, the santonine, and especially the santonine-soda, is transformed wholly, or in part, in the economy into a material which is discharged with the urine, and is detectible in the latter by means of caustic alkalies, which immediately redden the urine. This substance the author provisionally termed *Xanthopsin*. 5. This conversion of santonine into xanthopsin takes place, under certain conditions, in a very short time, while the elimination may be long continued. In one experiment the elimination commenced in thirty minutes; and in another, in which no large quantity of santonine was introduced, it continued during sixty hours. 6. Under the influence of santonine, the urine soon assumes a peculiar yellow color, dependent upon the same material, which is converted into a red color by the action of the caustic alkalies. 7. After the evaporation of urine contaminated with santonine, the red color ceases to be produced on the application of caustic alkalies. 8. As under the influence of santonine, saffron-colored urine is excreted, this may assume a red color in consequence of the extraction of ammonia by the decomposition of the urea, or, if alkalies are administered, with the santonine. This position is not, however, a result of direct experiment, but merely an inference. 9. Santonine exerts on the brain and visual organs a remarkable influence, inducing an incoherence of idea and chromatopsy.

For the production of the latter, large doses are always required. 10. The production of chromatopsy is in close connection with that of xanthopsin. The more xanthopsin mingled with the blood, the more marked is the chromatopsy. 11. A direct introduction of a watery solution of santonine-soda into the eye does not induce chromatopsy. 12. The symptoms of poisoning by santonine vary much, according to differences in proportions and conditions. Convulsions have almost always preceded the fatal results.—*Comptes Rendus*, tome 1, No. 17.

4. *Medical Excerpts.*

Pathology and Treatment of Chloroanæmia.—Dr. Eisenmann, from an extensive observation of this affection, concludes that it is especially developed under the influence of medical constitutions which predispose to nervous affections. It is especially met with at a time of life when all kinds of neuroses are very prevalent, its appearance being ushered in by nervous phenomena, at a period when as yet the blood has not undergone the slightest change. Such change may even be absent when the disease has reached its full development, nervous symptoms being observable, however, during its entire course. It is curable by agents which exert a special action on the spinal marrow; and when left to itself, it often gives rise to chronic, or even fatal, spinal affections. All these considerations lead the author to the final conclusion that chlorosis is a primary nervous affection, the change in the blood being a secondary phenomenon, due to morbid innervation. Guided by these views, he treated several cases of the complaint by means of tincture of St. Ignatius' bean, with great success. Wishing, however, to effect a more prompt recovery than that which takes place when the bean alone is given, he associated it with ferruginous preparations, adding also rhubarb, on account of the constipation which is usually present. The following is the formula he employs: Powder of St. Ignatius' bean, gr. i; lactate of iron, or iron filings, grs. iij; rhubarb, grs. iij to iv; and oleo saccharate of peppermint, grs. iv. This is repeated twice a-day. When the stomach is irritable, the iron is left out. This compound cures far more rapidly and effectually than do mere preparations of iron.—*Bull. de Thérap.* tome 1, p. 250.

Arsenic in Apoplectic Congestion.—M. Lamare-Picquot, physician to the Honfleur Hospital, as the result of ten years' observation and trial upon between forty and fifty cases, in-

cluding his own among them, strongly recommends the prolonged use of arsenic as an effectual means of subduing congestion likely to give rise to apoplexy. In very urgent cases, in which hemorrhage seems imminent, he precedes its employment by a moderate venesection, but this is quite exceptional. In proportion to the severity and menacing danger of the case the dose requires to be larger; and although, even after a month, benefit may already result, to be of permanent benefit it will have to be continued for several months. The more urgent the case, the more tolerant does the system become of the arsenic. The author, regarding apoplexy as consisting essentially in an excessive increase of globules of the blood, employs arsenic as a powerful agent for decreasing these, as well as the plasticity of the blood. It becomes, of course, necessary to assure oneself in a given case of the richness of the blood, for to employ arsenic when the blood is impoverished would be to do mischief. The author has generally found the dose of $\frac{1}{15}$ to $\frac{1}{6}$ gr. per diem sufficient.

Uselessness of Sarsaparilla.—Professor Sigmund, the Vienna syphilologist, has been conducting a series of experiments with carefully-prepared sarsaparilla, and has come to the conclusion that the employment of this substance alone in gonorrhœa or in primary or secondary syphilis is of no essential service; while the efficacy of other decoctions which contain it, especially Zittmann's, is not due to the fact of their containing sarsaparilla. He refers to the enormous expenditure which is incurred for this drug throughout Europe, and which he evidently regards as so much money thrown away, as far as syphilitic affections are concerned.—*Zeitschrift Aerzte der zu Wien*, 1860, No. 1.

Erysipelas of Infants—*Powder of Starch, Tan and Calomel.*—The dangers which attend the erysipelas of new-born children, especially when it originates in the umbilical cicatrix, are well known. In this case, says the *Gazette des Hôpitaux*, Dr. Legroux has for some time used, in his nursling ward at the Hôtel-Dieu, a method which appears to yield most satisfactory results.

It consists in smearing the diseased parts with glycerine, and applying a powder composed of equal parts of starch, tan and calomel. The dressing should be renewed two or three times in the course of the twenty-four hours.

Sedative Pills.—The following is the formula of the most efficacious of pills in the sleeplessness of hypochondriacs and

hysterical persons, and indeed of all persons suffering from nervous affections: Assafoetida one drachm, sulphate of morphia three grains, into thirty pills, one or two at bed-time. From two to four of these pills daily are of great use in relieving the dry cough to which nervous women with irregular menstruation are liable.—*Moniteur*, No. 97.

Croup without Croupal Cough.—Dr. Gottschalk relates some cases to show that while, on the one hand, a cough, precisely resembling that of croup, may be present in mere laryngitis, the characteristic cough may be entirely absent in true croup attended with fatal exudation.—*Journal für Kinderkrankheiten*, band xxxiv. p. 30.

Extract of Rhatany.—When prescribed in aqueous solution the extract of rhatany generally falls to the bottom. This may be prevented by adding to the pulverized extract a little water, and from twenty to twenty-five drops of alcohol—an addition, however, not required, when tinctures are prescribed with the extract.

Carbonate of Potash in Chronic Diseases of the Bones.—Dr. Pockels says he cannot sufficiently extol this as an application, in a concentrated form in chronic diseases of the bones, especially caries and necrosis.—*Varges Zeitschrift*, vol. xiv. p. 7.

SURGICAL PATHOLOGY AND OPERATIONS.

1. *Case of Absence of the Bladder.* By Prof. UYTTERHOEVEN.

A woman, aged 40, was admitted into the St. Pierre Hospital, Brussels, with tuberculosis. She had suffered from incontinence of urine since the age of twelve years. On examination there was found to be an urethro-vaginal fistula. She died from the phthisis in the course of a year. At the autopsy, to confine ourselves to the urinary organs, the right kidney was found tuberculous, its parenchyma having disappeared, and its thickened membranes distended with caseous matter alone remaining. Its ureter, obliterated during its lower third, had become condensed into a ligamentous cord.

The left kidney also contained some softened tubercles in its substance. Its ureter exceeded its normal calibre, was hypertrophied, and opened on a level with the urethro-vaginal fistula. The urethra was much larger than in the normal state. In front of the uterus, in the place which the bladder should have occupied, an utricule, the size of an ordinary pea was found, lined internally with mucous membrane. On this inner surface two minute apertures existed, through one of which a delicate probe could be passed into the non-obiterated ureter, and hence to the fistula. This last, small and rounded, was situated at the point of union of the upper and middle thirds of the vagina. At first sight this might have seemed an example of congenital absence: but the patient declared positively that prior to the age of 12, when she first began to menstruate, she held her water well. Tubercle had probably been deposited in the vesico-vaginal partition. Inflammation and suppuration followed, and an aperture was formed between the internal surface of the vagina and the anterior angle of the vesical trigone. The urine being expelled drop by drop by the fistulous opening, and accumulation gradually ceasing in the bladder, the organ in the course of the thirty years became atrophied. *Presse Medicale Belge*, No. 29.

2. Gun Shot Wound of Penis, with the Results.

Hugh C., aged 16, had been amusing himself shooting woodcock. Preparing to return home, he thoughtlessly attempted to place a pistol loaded with small shot in his breeches pocket; in doing this, it exploded, and produced a serious wound of the penis. The shot entered the body of the organ just in advance of the pubes, and frightfully lacerated it up to the end, splitting the glans into three peices, considerable portions of which subsequently sloughed. The bleeding was very difficult to stop, owing to the vessels having retracted in the cells of the spongy tissue; two vessels were tied, and the parts wrapped in cold cloths. There was some temporary stoppage of the urine, which required the catheter; on the second day he voided it freely, though it had a spiral course. There was, after a few days, much suppuration, but the wound healed rapidly; there was a great deal of lymph in the cellular structure, and the organ bulged very much in the middle, like a barrel; the glans was split to the centre, and the urethra opened below, producing a traumatic hypospadias. It was proposed to freshen the edges of the fissured glans and unite them by wire sutures, but the boy's disposition was intensely

conservative, he wished to retain all he had, without further risk. In four weeks he was entirely well.

3. *Surgical Excerpts.*

Compression in the Treatment of Varix.—Professor Botto, of Genoa, has of late treated varix with success by applying compression at two points along the course of the saphena vein—a procedure, he thinks, very preferable to puncturing or injecting the vein. At first he employed digital compression, but afterwards he substituted his present plan of making it. The patient first assumes the standing position for a long period, in order to induce as much distension of the varices as possible. Two pellets of charpie are then fixed firmly by means of two circular rollers over the saphena interna vein, the one above, and the other opposite the knee, in such a way as to comprise between them an interval of about sixteen centimetres. Some inflammatory action is at first produced, but this soon subsides, and in the course of a month the vein becomes obliterated, and reduced to the state of a hard, compact cord. The large varices between the knee and groin will have entirely disappeared; but to obtain a complete cure of those of the leg, and back of the foot, a new compression will have to be instituted at the lower third of the leg.—*Rev. Méd.* 1860, vol. i. p. 369.

Bismuth and Glycerine in the Treatment of Burns and Scalds.—Professor Richardson, of the Charity Hospital, New Orleans, states that this combination constitutes the best application he has yet met with. The bismuth is to be rubbed up in a mortar with a sufficiency of glycerine to form a thick paste, which should be applied by means of a camel's-hair pencil, or a soft linen mop. Previously to employing it, the parts should, if possible, be thoroughly dried, to which end it is necessary to prick with a needle any blisters that may exist, and carefully wipe the surface by gently pressing it with a piece of dry lint. A thick coating having been applied, the parts should be protected from friction by a sheet of clean carded cotton. In very slight burns, in which there is erythematous redness without discharge, dry bismuth may be dusted on, the secretions of the skin forming with it a pasty, protective coating.—*North American Med.-Chir. Review*, July, p. 656.

Transfusion after Exhausting Suppuration.—Dr. Neudörfer, of Verona, gives here an account of some trials he has made of the efficacy of transfusion in some cases in which exhausting

suppuration rendered death imminent. The subjects were soldiers, who had suffered from wounds in the late Italian war. He has tried the practice in six cases, and always found a remarkable temporary improvement result. The pulse regained some force, refreshing sleep (unprocurable heretofore by opiates) was obtained, the appetite increased, and the severity of the pain diminished. This improvement continued for from five to eight days, when the former unfavorable condition recurred, and death resulted within three weeks.

Formulæ for Creosote.—M. Lebert employs a lotion consisting of 1 to 4 parts of creosote to 1000 of water, as an application in burns, and in putrid or cancerous ulcers; and in the treatment of wounds and ulcers M. Guibert applies charpie, soaked in a mixture composed of four ounces of glycerine, and twelve drops of creosote.—*Bull. de Thérap.*, July, p. 26.

OBSTETRICS, &c.

1. *Urticaria as a Symptom of Irritation of the Female Sexual Organs.* By Prof. SCANZONI.

Professor Scanzoni observes that although it has long been known that chronic affections of the female sexual organs are not infrequently accompanied by skin diseases (as urticaria, eczema, acne, psoriasis, chloasma, etc.,) the influence of a more sudden irritation of these organs upon the cutaneous surface is by no means so well established. He has been enabled to find no very definite statements upon the subject, and this leads him to communicate some cases tending to establish such a consensus.

A lady, aged 34, had been under his care for sometime with slight retroflexion of the uterus and chronic metritis, when he ordered four leeches to be applied to the vaginal portion of the cervix uteri. This little operation had been already performed once before without any ill effect, but upon the present occasion, ten minutes after the application had been made, the patient was seized with violent febrile action, and slight delirium. In half-an-hour she was seen by the author, who found her skin, and especially that of the face and upper part of the body, almost of a scarlet red. The

temperature of the surface was considerably raised, and her pulse beat 136. She continued much the same during the night, and when seen next day, the face, neck, chest, arms, and thighs exhibited, together with the intense redness, innumerable urticaria elevations. In a day or two the exanthem had entirely disappeared, a distinct desquamation, however, taking place on the face and neck. As this was the first case the author had ever seen in which these symptoms followed the application of the leeches to the cervix, he did not believe in their dependence upon this, and again ordered them to be employed. Four times this was done without any unpleasant occurrence, but on the fifth occasion the whole series of symptoms above described were reproduced, and that so rapidly after the biting of the leeches that any doubt as to cause and effect could no longer be entertained.

In a second case, a woman, aged 28, was admitted into the Würzburg Midwifery Institution on account of chronic uterine infarctus, and five leeches were ordered to be applied to the cervix. Scarcely had they taken hold, when she complained of the most violent labor-like pains in the abdomen, and although these soon moderated in force, they were accompanied with such intense febrile action that the entire body glowed with heat, the pulse rose to 140, the carotids pulsated visibly, and the face, neck and chest exhibited an intensely red color, to which were added in a very short time a large eruption of urticaria elevations of a palish color. The eruption was accompanied by great headache, inclination to vomit, and excessive lassitude, symptoms which continued to the following day, although the exanthem with the accompanying fever disappeared entirely after three hours' continuance. This patient often had suffered from urticaria at the menstrual periods, without, however, its being accompanied by such violent symptoms.

The third case occurred in the person of a young lady, aged 26, who, on account of long-continued chronic oöphoritis and metritis, required blood-letting. In the course of sixteen months four or five leeches had been applied eight times. On the ninth occasion, an intense redness covered the skin, and the patient complained of the most violent pain in the head. The temperature of the surface was much raised, and it was almost entirely covered with innumerable, minute, prominent, white elevations. In the course of an hour these appearances gradually subsided, the headache continuing for twenty-four hours longer. The author is aware of a fourth case of the same kind, but is unable to furnish the particulars.

Professor Scanzoni believes that these cases deserve the attention of those occupied with the diseases of women, as

well as of dermatologists. They admit of no other explanation than that the irritation of the uterine nerves, caused by the bite of the leeches, induced an entirely unusual, and in its mode of origin inexplicable, disturbance of the vascular system, which again, in a mode which is to us equally unintelligible, gave rise to the production of the eruption of urticaria. In proof that these appearances were not produced as a consequence of any poison being conveyed through the medium of the bite of the leech, it is to be observed that similar symptoms never result from the application of leeches to other regions of the body, while it is to be observed that even very slight irritation of the sexual organs, as that produced by examination with the finger or speculum, or by the application of caustic, will in many very sensitive women give rise to erythema of the face, neck, breast, etc., which disappears as rapidly as it comes on. *Würzburg Medicin. Zeitschrifts*, band i. pp. 90-95.

2. *Pepsine in the Vomiting of Pregnancy*.—M. Corvisart, while communicating a case furnished him by M. Teissier, in which pepsine proved of most marked efficacy in very obstinate vomiting in pregnancy, furnishes additional testimony to the value of this drug in these serious cases. Several others, too, have recently been published in the French Medical Journals. M. Corvisart points out the value of pepsine in either proving useful at once, if, as is often the case, the vomiting is due to an altered condition of the gastric fluid, or in showing itself completely inefficacious when the vomiting is dependent upon sympathetic muscular irritability of the stomach. Either relief or an important diagnostical assurance is at once obtained. M. Corvisart repudiates many of the formulæ of pepsine, which have of late obtained currency, approving, however, of those of Baudault and Mialhe. Still, he exclusively employs the powder, or when the stomach will bear alcoholic fluids, vinous elixirs of this substance.—*Union Med.* No. 44.

3. *Treatment of Leucorrhœa*.—In leucorrhœa from chronic stasis of the uterus, without considerable textural changes and in the absence of syphilis, Dr. Pockels has for a long time past administered with great success *secale cornutum* and *catechu*, giving of each as much as will lie on the point of a knife three times a-day—the *catechu* being as serviceable as the more expensive tannic acid. If there is anæmia, phosphate of iron is added, and alkalis when acidity of the stomach prevails. An increased secretion of mucous is at first produced, and this may have some blood mixed with it when chronic hyderæmia is present.—*Varge's Zeitschrift*.

BIBLIOGRAPHICAL RECORD.

Transactions of the Medical Society of the State of Pennsylvania, at its Twelfth Annual Session, held in Philadelphia, June, 1860. New Series, Part V.

WE have here a respectable volume of "Transactions," filling nearly two hundred pages, octavo, with matter which is for the most part both interesting and valuable.

The President of the Society, the distinguished Dr. Condie of Philadelphia, delivered an address before that body at its annual session, in which he dwelt forcibly upon the great advantages derived from the principle of association. We fully agree with him. In our profession, nothing tends more to the elevation of tone and the diffusion of sound practical knowledge, than frequent conferences among those members who honestly strive to attain to excellence. When many intelligent men agree to bring the fruits of their labors into a common stock, they are making a compact the benefits of which can scarcely be fully estimated. The first person improved is the observer, whose diligence will be stimulated by the fact that his work will be brought under the notice of intelligent, and perhaps critical, *confrères*; next, these gentlemen are improved by getting so easily at the experience of a good observer; then, from this association, be it large or small, an influence extends to the members of the profession generally; and last, but not least, the entire mass of society, in every sphere, is vastly benefited by having in its midst a corps of physicians equal to the demands of their most arduous, but most honorable calling. And who so high, or who so low, as not to need the physician's ministry? In all ages gaunt disease, as well as death,

"—— *Æquo pulsat pede pauperum tabernas,
Regumque turres.*"

And so the true physician enters every home, from the house where his eyes are dazzled with splendor, down to the most humble hut where he sees nothing but the nakedness of hopeless poverty; and in each he is the minister, the oracle, the friend, upon whose skill depend the hopes of one suffering fellow-creature and his circle of friends. Sometimes the friends say earnestly, the very sight of the doctor improves the patient; undoubtedly it does, and happy is the physician whose character inspires such confidence, and happy the patient who has such a physician.

Association and interchange of knowledge acquired, make the ordinary physician a clever one, and make the superior physician a master. Thoughts may be better expressed in words spoken than in books, which admit of no searching cross-questioning; and in conversation, a few well informed physicians can often transmit and receive, to and from each other, clearer views of important subjects than can be conveyed through more formal channels.

As physicians have common interests, scientific, pecuniary and social, many think they should petition legislative bodies for statute laws in their behalf. We do not believe that legislatures would have either the power or the will to render us any special service. The influence and prosperity of the profession must rest principally upon the character of its members. For ourselves, we know of nothing better calculated to elevate and support this character than well ordered association. Let the physicians in each community enrol themselves in a society, the members of which will pledge themselves *in all matters of common interest*, to act in concert. Let the society exact of each member to conduct himself in a manner worthy of a physician and a gentleman, under penalty of being ruled out, and not received or met in consultation. A court of honor, or a board of censors, might decide differences between physicians, so that parties charged, or preferring charges, should have respectively, impartial hearing. Let there be stated meetings for mutual improvement.

In large towns and cities there might be, very well, medical

clubs, where the associates could meet upon equal terms, and where strangers belonging to the profession could meet their brethren resident. In Baltimore and Richmond, for example, there might be club-rooms where all the members of the State Societies could meet whenever in the city as a matter of right, and where all other *medical gentlemen* might be received by courtesy. In rendering civility to strangers, such societies should know no North, no South, and no nationality in fact; the only necessary qualification to admission being good professional and personal character. All the local societies should be affiliated to the respective State societies; each member should contribute a small sum for the good of the general organization, and in return for his subscription, besides advantages always to be enjoyed in his own society, or at headquarters, he should be supplied with a medical journal; most appropriately, in the States of Maryland and Virginia, with this Journal, since its origin and growth are especially under the fostering patronage of these two sister States. Thus every contributing member would have something brought home to him of appreciable value, over and above the general advantages already mentioned.

The broad suggestions here thrown out could doubtless be advantageously modified and improved on; we were led on to make them while only intending to express an acceptance of the principle so ably advocated by Dr. Condie. We are ready to concur in any well devised arrangement for the common good which may seem acceptable to our professional brethren.

There are many other matters in the Transactions worthy of notice, more in fact than we have space to notice. Professor Gross supplies a valuable paper on the "*Nature and Treatment of Prostatorrhœa*," a disease which has been overlooked by the profession, because the discharge from the prostate gland has been confounded with various other discharges from the genito-urinary organs. It is often mistaken for spermatorrhœa, and gives the patient the same kind of moral distress, with some of the same symptoms; but the discharge never shows spermatozoa under the microscope, and may thus

be distinguished if more obvious symptoms are indefinite. The treatment depends upon tracing the irritation or inflammation of the prostate to its cause, the *fons et origo*, upon removing the cause so far as may be, and then, upon general principles, with the use of laxatives, tonics, as *tinct. ferri mur.*, and *tinct. nucis vom.*, and certain local applications. Prof. Gross expresses a preference for injections containing "Goulard's extract with wine of opium in the proportion of one to two drachms of each to ten ounces of water, thrown up forcibly with a large syringe three times a day, and retained three or four minutes in the passage." This failing, cauterization may become necessary.

In the county reports, we find, as may be expected, a good deal of loose statement with not a few valuable facts.

Some of the physicians in country practice do not believe in the universal change in the type of disease, with the modification of treatment, so generally admitted by the profession. We take the following passage from the Report of the Montgomery County Medical Society :

"*Blood-letting.* The use of the above remedies, though highly approved by the members of our society, has not, we think, lessened the use of the lancet. On the contrary, it has dissipated the too prevalent belief that diseases have changed so greatly—become so much less inflammatory—that they will not bear depletion. They say, if the system will bear such prostrating doses of those medicines (*verat. viride*, and *digitalis*) for days, and then re-act when they are discontinued, it will not surely give way under the loss of a few ounces of blood; and the facts confirm the reasoning. Some of our members who have practiced more than a quarter of a century, use the lancet freely, and assert that depletion by it is as well borne as thirty years ago. Indeed, not a few are inclined to believe that many, particularly children, are lost for want of its use. Mercurials are used much less frequently and profusely than formerly, and there are few cases of disease in which they are considered indispensable."

Similar views are expressed in other reports. We believe they are based on sound observation. We are individually not at all of a sanguinary disposition; yet we think there has

been an excessive re-action against the use of the lancet. It is not long since we attended, with Dr. John Buchler, of this city, a gentleman from the West, who came here in the last stage of a fatal attack of pneumonia. He had been for several weeks under the charge of homœopathic practitioners, and had been solely under their mode of treatment. Neither of us hoped to save the patient when called, but we agreed in this, to wit, that in all human probability copious blood-letting in the earlier stages would have saved his life. We are far from asserting that blood-letting is always necessary or proper in pneumonia; but, in this case, the gentleman was naturally stout and plethoric, and would have been, in the nature of things, a most fit subject for that kind of treatment.

The views that we hold upon matters of this kind are most admirably expressed by Professor Lawson, of Cincinnati, in an article "On the Treatment of Inflammation, with special reference to Pneumonia," in the January number of the "American Journal of Medical Sciences."

We will take a few paragraphs from this article, which, in its totality, is a criticism upon the views of Dr. Bennett, of Edinburgh. Dr. Lawson contends, and proves, that statistics have no certain value, and that the type of disease is ever varying, and ever demanding suitable modifications of treatment:

"If we seek an explanation of these contradictory results (previously exhibited) in the treatment of pneumonia, it will be found in the numerous qualifying conditions connected with age, season, climate, epidemic and endemic influences, early treatment, stage, extent and complications of disease; and to these conditions we must add, in a general sense, the *individuality* of each case; indeed, so great are the differences in constitutions, that no two examples will exhibit the same characteristics throughout, nor will they admit of precisely the same method of treatment. And it is the due appreciation of these more minute shades of differences, as well as the broad distinctions observed in the varying *forms* of the disease, that constitutes the truly skillful physician, and which enables him to meet the emergencies of each case, instead of relying on conclusions drawn from *groups* of cases.

“Viewing nationalities in a somewhat prejudiced light, a critical writer intimates that the English think more of some other case than the case under treatment, while the French think more of the disease than of the patient; hence, the former individualize the disease, the latter generalize the patient; but the true course is that indicated by Hufeland, to *generalize the disease, and individualize the patient*. Statisticians rob each case of its individuality, and cast it upon the sea of uncertainties pertaining to others of a different character. Thus, one series will all be bled, another will receive tartar emetic, and a third left to the chances of nature. In the first class, some are bled who should have been stimulated; in the second, tartar emetic is administered when bleeding would have been preferable; and, in the third class, some are permitted to die from mere over-action. In this blundering, if not criminal procedure, individuality is ignored, and the practitioner prescribes for a mere *name*, leaving the patient to the mercies of chance or fate.

“It is evident, therefore, that a rational treatment must secure to each case its own individuality.” * * * * If this be not a sound view of the philosophy of medicine, we know not where to look for it.

Our readers, perhaps, will excuse us for quoting a little further from this judicious article. It must be understood that Dr. Lawson is not over partial to the lancet:

“At the same time I am strongly inclined to believe that the great outcry against bleeding has driven us to the opposite extreme, and we now deplete less than the interests of our patients frequently require. With the prevailing aversion to bleeding, cases are liable to be overlooked, and depletion neglected from sheer habit. Dr. Christison clearly proves, within his own personal experience, that the synocha of Cullen has several times recurred, and each time demanding depletion. But he who would regard that form of fever as a *myth*, would not recognize its new introduction, and, therefore, would fail to meet its exigencies.”

Our American professor concludes his articles with a homethrust at him of Edinburgh:

“But let us hope the end is not yet. Theory and practice do not always accord. The celebrated blood-funker has been

attacked in a tender way. Inflammation came stealthily upon him, when, lo! Dr. Bennett's case *d demands blood-letting!* His colleague, Professor Miller, informs us that his sthenic constitution nobly sustained depletion. Thus, the hand of Providence becomes a more potent teacher than statistical tables or microscopic revelations."

We have no sort of doubt that Dr. Bennett usually pursued the right practice in not bleeding his morally and physically depraved patients, in pneumonia, as they came under his charge in hospital; and surely Dr. Miller was not wrong when he let blood from the *sthenic* system of his hale and well-fed brother professor. In a due appreciation of the differences, lies the whole philosophy of successful practice.

We can touch but a few of other topics treated of by our brethren of Pennsylvania in the "Transactions." In the report of the Bedford County Medical Society we find this passage:

"OBSTETRICS.—In this section of country, where we have plenty of fresh air, and the female feels herself not above labor, which invigorates and perfects all the functions of the system, and particularly the uterine, their labors are probably comparatively easy."

We do not doubt the accuracy of this view, and yet it is opposed to our experience, as expressed in the *Obstetrical Memoranda*, supplied by us to the October number of this Journal. We then stated that, in attending upon primiparæ, we met with notable deviations from normal labor in a third of our cases, and that, in nearly every instance, the deviation was among the working classes. We think, however, a comprehensive view of the facts may explain the apparent contradiction. The country-woman is invigorated by work in a pure and bracing atmosphere, with simple habits of life; whereas, the town-woman, with the relaxation of system produced by the impure air of cities, by tight dressing, and habits of life more or less irregular and artificial, is rather injured than invigorated by laborious occupations. Thus, the one brings her children naturally and safely; while the other brings them with more

than the average of suffering, which devolves on all the daughters of Eve.

In the report of the Indiana County Medical Society, we find that sudden deaths have attracted attention there, as well as in so many other communities :

“HEART DISEASES.”—During the past year, there were more heart affections than has been known before. Many of them proved fatal—some very suddenly. There was one peculiarity about most of the cases—a large number of them were among those in comfortable circumstances; that lived well, the most industrious, and generally of regular and temperate habits.”

If there is any one habit that tends more than others to the development and fatality of heart disease, we believe it to be *excessive eating*. This is a form of intemperance that is almost universal, and yet is not properly reprobated. When the stomach and bowels are always loaded with as much as they can carry, of course there must be more or less obstruction to a free circulation of the blood: indigestion, short breathing and functional disturbance of the heart ensue; and if the warning is not taken, organic disease and sudden death may ultimately be expected. There are worse habits than over-eating to be sure, but then they produce their effects more obviously, besides being often superadded to the one, which, insidiously, produces so much evil:

“*Hinc subitæ mortes, atque intestata senectus.*”

The evil is not new, but it is on the increase.

We must bring this paper to a close. We have taken passages at random from the “Transactions” to comment upon, and we do not know that we have by any means fallen upon the most interesting subjects; but we have shown to our readers that the Medical Society of Pennsylvania is zealous in promoting science and mutual improvement; and we may add, we would wish to see the Societies of Maryland and Virginia, equally or more zealous. The field is one of honorable ri-

valry; and from personal acquaintance, and the reputation of medical men that circulates abroad *viva voce*, we are sure that our two States contain a large number of gentlemen who are the peers of any practitioners in any State or in any country.

R. McS.

BALTIMORE, Oct. 11, 1860.

EDITORIAL AND MISCELLANEOUS.

"CURRENTS AND COUNTER CURRENTS."

We continue to hear of Dr. Holmes' lecture everywhere. All those whose interest prompts, or whose taste leads them to ridicule the medical profession, (and they are many,) are seizing the opportunity here given them with hearty good will. The daily and weekly newspapers and the literary serials are republishing it with comments extensively; and quacks of all sorts, as the homœopaths, the water-curers, the electrical men and advertisers generally, are making every possible use of it they can. During the perusal of this pamphlet, we could not help calling to mind the classic myth of the cunning Mercury stealing away the oxen kept by Apollo, of his driving them away *backward* for the purpose of better escaping detection, and of his filching the very arrows which the God of Poesy and Medicine had intended to direct against him in anger. When we had finished, the sly young Mercury, by a strange kind of metamorphosis, was changed into a wrinkled Momus, grinning grotesquely, and finding fault with what the world has ever considered most admirable.

But what can be expected for Medical Science from "The Autocrat of the Breakfast Table?" His reputation for origi-

nality and wit, established among a small coterie, must be supported at all risks and at the expense of things most precious and holy. A clever man making several thousand dollars a year by itinerant lecturing, and whose business it is to hunt up new ideas and pander to the popular taste, can ridicule anything. But ridicule is neither argument nor truth. We suppose the lecture was prepared without proper reflection or forethought of consequences. We are willing to believe that it was intended exclusively for a medical audience, where all could duly appreciate the Doctor himself—approve or disapprove of his positions, and qualify assertions, which seem to the unprofessional sweepingly to condemn all the appliances of medicine.

There are works strictly professional in all the professions, and when used or reviewed outside of these professions their proper spirit is frequently perverted and their tendency more likely to be injurious than beneficial. If this was the case with the present address, after being delivered before the Massachusetts Medical Society, it should not have been published. We saw no announcement upon it saying that it was requested for publication. If, therefore, this was a gratuitous straining after effect by the Doctor himself, he cannot blame the Profession for defending themselves in the eyes of those who have made a bad use of it. If it was published "according to custom," the Society must bear the blame of publishing what it refused to endorse.

We are not acquainted with the real sentiments of the author, and, such has been the habit of his mind, that we doubt if, in the excitement of writing, he is always acquainted with them himself. It is a recognized metaphysical fact, that satire and wit are highly cultivated only at the expense of other and better qualities of mind. Truth is often sacrificed to a witticism, and reality distorted into cunning analogies. No age or language has produced a satirist who has not given the medical profession his fullest attention. In the treatment of the multiplicity of diseases of the body and mind, it would be wonderful if there were not many points salient to the shafts of a keen-

witted scholar. It is a well established fact, recognized alike by the physician, metaphysician, and the psychologist, how pathological conditions increase faith. As Dr. H. is not a representative of practical medicine, his sentiments would probably undergo a complete revolution if he conscientiously took charge of a large practice for one week, but more certainly, and more completely still, if feelingly approached with the "argumentum ad hominem." When the otherwise invincible sceptic is fairly met in this way, as in the instance of a syphilitic iritis, a spasmodic stricture of the urethra, a passing gall stone, an impacted rectum, an ulcerated pharynx, or one of the thousand ills of flesh, we find confidence established, faith restored, and the whole man thoroughly changed, with his face bowed towards the Æsculapian temple an humble and believing worshipper.

Having these facts daily and hourly before us, we confess that we have never for one moment lost confidence in the principles of medicine, or in the virtues of the remedial means and agents used in the healing art. The physician's studies peculiarly acquaint him with the fact that the avenues to skepticism are numerous and broad. As a production in different minds, it can claim multifold parentage; but logic, good common sense, or fair observation never beget it in the practice of medicine. If the skeptical system subverts the whole foundation of morals, surely no philosophy, much less our own, will be advanced by the sophistries of the Pyrrhonist. "Tell me your conclusions," remarked an intelligent friend recently, "of doubts I have enough of my own."

When we divest this lecture of all its outside wrappings and brilliant adornments, and examine it in its naked reality, we find in it, as in its archetype from the pen of Dr. Forbes,* the individual assertion that art has not and never can triumph over disease. The latter has not given to the disease the advantage of having a nature of its own, and neither have strictly and *properly* defined that nature which they eulogize and call upon mankind to substitute for art. No true advancement will

* Nature and Art in Disease.

ever be made in medical science as long as we flounder with the Doctor among the bogs and quagmires of unbelief.

For our own parts, we would a thousand times rather contribute one small stone, one atom towards the erection of the unfinished temple of science, than to engage in the odious task of endeavoring to tear down or undermine an edifice on which so many master-builders have expended the best efforts of their lives. And even could he accomplish his purpose, pray what does he offer as a substitute for what he wishes to destroy? A sentence, a vague abstraction, an indefinite generality which may mean anything, and in reality means nothing—viz., “we of the school of nature.”

Long may it be ere the Doctor's sprightly face is contorted with the aching nerve, or his blood (like Dives faring sumptuously every day) be so loaded with lithic acid, that he can no longer enjoy the luxuries of wealth; but when this time does come, and, if he lives long enough, it must, may his lively cerebrum be sufficiently spared to give us the other and *truer* side of the picture.

MEDICAL ASSOCIATION.

The distinguished President of the Pennsylvania Medical Society found it necessary to complain of a want of zeal amongst the members of that Association at its last meeting. His address was calculated to arouse those present to a sense of their duties to themselves and to the Society which should be a part of their professional existence. Notwithstanding this want of zeal, to a valuable report of transactions is appended a list of more than eighty names of members present. We do not know what the proportion may be for the two States of Pennsylvania and Maryland, but we mention the fact (in sorrow) that at the time advertised for a meeting of the Maryland Medical Society, in June last, one less than a quorum, which is fifteen, was the largest number that assembled together at one time. In the year 1860, there has been and will be no meeting of the Maryland Medico-Chirurgical Society. We believe this is

the first failure to obtain a quorum at an annual meeting which has happened to this Society since its organization, nearly half a century ago. Although so few in numbers, there was no lack of ability present. The underlying causes of the want of zeal in the members was the subject of an interesting conversation whilst waiting for the one man to arrive, in order that the quorum might be completed. So much were we impressed with this conversation, that we have since frequently urged some of the members of the medical profession in Maryland and Virginia to prepare a series of articles upon the subject of medical association—setting forth its advantages or disadvantages, and proposing some plan of organization should the advantages be found to preponderate. It was unanimously admitted in the conversation referred to, that an entire re-organization must be effected in order to arouse a vitality in the Society. It was asked, “What should be the basis of the re-organization?” Upon the proper answer to this question depends the usefulness and life of the Society for the future. Whether it is better in this country for the members of the medical profession to rely entirely upon their own individual merits for success and for attaining the ends of their ambition, or to form associations, a membership of which will be an evidence to themselves and to the world of their ability and character. At the present time, some of the most esteemed practising physicians of Maryland do not belong to the Medical Society of the State, or to any medical association. They are entirely alone in their positions, and have only the merit of having obtained and retained a large practice to reward them for their labors. They have no endorsement from those who have pursued the same line of study how these labors have been directed. Would it not be a well-directed and a high-aimed ambition to obtain such an endorsement, and a grateful feeling after their labors are completed to have obtained it?

Is not an active participation in some of the royal colleges abroad something to be sought after?

And what prevents every State in this Union from forming

some elevated society, to which it will be a present and lasting honor to belong? We were glad to hail the remarks of the gentleman who reviewed the Transactions of the Pennsylvania Society in our columns. We know that they were only intended as a beginning, and hope they will be continued by all those who may feel an interest in the subject, or are impressed with its importance.

THE MEDICAL COLLEGE OF VIRGINIA.

The alumni and friends of the Metropolitan Medical School of Virginia will gladly hear of its increasing prosperity and enlarging sphere of usefulness. At this early period of the session, more than *one hundred and thirty students* have matriculated—the largest medical class ever assembled in Virginia.

The wise liberality of the Legislature, in its recent appropriation of thirty thousand dollars to the Medical College of Virginia, already yields a noble harvest, and with the earnest determination of its Faculty to fail in no effort which their responsible position demands of them, we cannot hesitate to believe that a perfect and enduring success will be attained.

The new year will witness the opening of the beautiful and convenient Hospital now being erected on the grounds of the College, offering ample accommodations to the sick and additional clinical advantages to the student, who is thus enabled to familiarize himself with practical duties of his profession under the most favorable circumstances; the facilities for imparting instruction in all the branches of a medical education are on the most complete and liberal scale; whilst recent importations of models, casts and apparatus in every variety, enable the lecturer to illustrate his subject with all the resources of modern art.

THE UNIVERSITY OF MARYLAND.

This Faculty commenced their course of lectures on Monday, the 15th of October, with a general introductory by Dr. Edward Warren, Professor of Materia Medica. In his exordium, he paid a becoming tribute to the memory of those three gentlemen—Professors Joseph Roby, Charles Frick and Dr. B. B. Smith—who have died since the closing of the last course of lectures. His subject was “a defence of medicine.” We forbear further comment at present, other than a most hearty approval of its sentiments, together with an expression of the great pleasure we experienced during its delivery. The lecture was enthusiastically received by the entire class, and we doubt not a copy will be requested for publication—with which request we hope the author will comply.

Professor Hammond delivered his first lecture on Tuesday. Owing to a regulation of the Faculty for some years past, only one general introductory to the course is given. The rest are special introductions to the several chairs. Prof. Hammond’s was a scientific discourse concerning the ultimate elements of the body. His style was clear, and his manner impressive and pleasing.

This flourishing school is now in full tide of successful medical instruction. The Faculty is complete, and we heartily congratulate its senior members upon securing the services of such accomplished gentlemen.

MEDICAL APPOINTMENT.

Dr. J. H. Butler, who, for more than three years, performed so satisfactorily the duties of Resident Physician at the Baltimore Infirmary, has been appointed by the Faculty, Demonstrator of Anatomy in the University of Maryland.

ARMY NEWS.

The Board of Army Surgeons, which assembled in Baltimore on the 20th of September, was composed of Surgeons C. A. Finley, Chas. S. Tripler, and N. S. Jarvis, with Assistant Surgeon Charles. H. Smith, Recorder.

Sixteen candidates were authorized to present themselves for examination. Of this number eight failed to appear or withdrew, and eight were examined in full. Of this latter number five were found qualified, and have been appointed Assistant Surgeons in the Army of the United States. The names of the successful gentlemen are as follows in the order of their relative merit :

Dr. Campbell Short, of Maryland ;
Dr. A. Francis Mechim, “
Dr. Clinton Wagner, “
Dr. David P. Ramseur, of North Carolina ;
Dr. William F. Cormick, of Virginia.

Drs. Mechim and Wagner are graduates of the University of Maryland, the latter having been clinical clerk at the Baltimore Infirmary for several years past. Drs. Short and Cormick are graduates of the University of Pennsylvania, and Dr. Ramseur (*we understand*) of the University of New York. The Board also examined Assistant Surgeons P. G. S. Ten Broeck, Lyman H. Stone and Edward W. Johns for promotion, who were all found qualified. Having completed the business before it, the Board adjourned *sine die* on the 6th of October.

Owing to the small number of candidates examined, the Board was not able to obtain a sufficient number of qualified gentlemen to provide for any vacancies which may happen within the coming year, and consequently another Board will be convened in the spring. Candidates desirous of appearing, should make application to the Secretary of War at Washington, and forward to him testimonials in regard to moral character and good standing.

PHYSICAL EXAMINATION OF CANDIDATES

For Admission, as Midshipmen, into the Naval Academy at Annapolis, Maryland.

The Medical Board for the examination of candidates for admission into the Naval Academy, which convened on the 20th of September last, was composed of Surgeons J. C. Palmer, W. Grier and Charles Eversfield of the U. S. Navy. It adjourned on the 5th of October, after having examined 126 candidates, of which number 7 were rejected and 119 found qualified.

The following are the rules established by the Navy Department for this examination :

“No candidate will be admitted into the Naval Academy unless he is found, in the opinion of a Medical Board, to be composed of the Surgeon of the Naval Academy and two other medical officers, to be designated by the Secretary of the Navy, qualified to discharge the arduous duties of an officer of the Navy, both at the time of his examination, and, probably, during the rest of his life, until age shall disable him.

Any one of the following conditions will be sufficient to reject a candidate, viz :

1st. Feeble constitution and muscular tenuity; unsound health, from whatever cause; indications of former disease; glandular swellings, or symptoms of scrofula.

2d. Chronic cutaneous affections, especially of the scalp, or any disorder of an infectious or immoral character.

3d. Severe injuries of the bones of the head; convulsions.

4th. Impaired vision, from whatever cause; inflammatory affections of the eyelids; immobility or irregularity of the iris, or fistula lachrymalis.

5th. Deafness; copious discharge from the ears.

6th. Loss of many teeth, or teeth generally unsound.

7th. Impediment of speech.

8th. Want of due capacity of the chest, or any other indication of a liability to pulmonic disease.

9th. Impaired or inadequate efficiency of one or both of the superior extremities on account of fractures, especially of the clavicles, contraction of a joint, extenuation or deformity.

10th. An unnatural excurvature or incurvature of the spine.

11th. Hernia.

12th. A varicose state of the veins; of the scrotum and spermatic cord, (when large,) sarcocoele, hydrocele, hæmorrhoids, fistulas.

13th. Impaired or inadequate efficiency of one or both of the inferior extremities on account of varicose veins, fractures, malformations, flat feet, lameness, unequal length, bunyons, overlying or supernumerary toes.

14th. Ulcers or unsound cicatrices of ulcer likely to break out afresh."

Within the last five years 536 candidates have been examined by the Medical Board—of which number 30 have been rejected as unfit for the service, being nearly six per cent.

The disqualifying causes were :

Malformation of chest,	2
Chronic cutaneous disease,	2
Myopia,	4
Pulmonary disease,	2
Disease of heart,	1
Hæmorrhoids,	1
Hydrocele,	1
Impaired efficiency of upper extremities, from fractures,					2
Feeble constitution and muscular tenuity,	1
Abnormal enlargement of inguinal rings—liability to hernia,					1
Varicocele,	13
					<hr/> 30

Candidates are required to be over fourteen and under eighteen years of age. The average age of those who enter the Academy is, approximately, estimated at sixteen years and six months.

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ART. I.—*Notes of Clinical Lectures, delivered at the Infirmary of the Medical College of Virginia.* By Prof. DAVID H. TUCKER. Reported by THOMAS L. HUNTER, M. D., Resident Physician.

1. LECTURE ON DROPSY, WITH CASES.

CASE I. "*Ascites.*"

JOE; a negro man; aged 40; admitted Sept. 20th, 1860. This man has for several years been employed as a hack driver. While acting in that capacity he was accustomed to take large quantities of whiskey, as he said, "to protect him against the weather." Two months ago he was attacked with violent palpitation of the heart and shortness of breath. This lasted for several weeks, when his abdomen began to be distended with an effusion of serum in its cavity. When admitted, his abdomen was very much enlarged. By percussion and fluctuation the presence of a fluid could easily be diagnosed. There was slight hydrothorax, with some anasarca swelling about the feet. The heart was examined, and valvular obstruction of the left side of the heart was discovered. He was *Ordered*,
R.—Hydrarg. chlor. mit., gr. xx.; pulv. scillæ, gr. xv; pulv.

digital., gr. xv. *M. ft. pill. No. xx. Sig.*—One to be taken three times daily. On the 21st, he was no better. The previous treatment was ordered to be continued, and a blister to be placed over the region of the heart. After the blister had drawn, and the pills of calomel, digitalis and squills had been continued for several days, the uneasiness about the heart and all dyspnœa disappeared. On the 27th, his mouth had become a little sore. He then expressed himself as feeling much more comfortable than he had since being taken sick, though the abdomen was very little, if any, softer or smaller than it was when he was first admitted. Did not pass a pint of urine during the day. The pills were ordered to be stopped. *R.*—Ol. juniperi ʒj.; ol. terebinth. ʒij.; spts. æther. nit. ʒss.; gum mucilage ʒij. *M. et sig.*—Take ʒss. three times daily. Keep the bowels open by giving once daily. *R.*—Pulv. jalap, gr. xx.; potass. bitart. ʒij. *M.*

Sept. 29th: He was no better. Kidneys not acting at all scarcely. Besides continuing the previous diuretic mixture, there was *Ordered*, *R.*—Spts. æther. nit. ʒj.; infus. carroti. sem. ʒij. *M. et sig.*—Ter in die. By the 8th of October the effusion had greatly increased in both the abdominal and thoracic cavities. His breathing was exceedingly disagreeable and distressing. Could not lie down. Had to be propped in a sitting position in bed. All previous treatment was ordered to be stopped. *R.*—Digitalis ʒj.; aq. bull. Oj. *M. ft. infus. Sig.* Give a table-spoonful ter in die. *R.*—Magnes. sulph. potass. bitart. aa ʒj.; aq. font. Oj. *M. et sig.*—Take a wine-glassful every four hours. *Give ʒvj. per diem.* Having been under medical treatment nearly a month without being benefited, he was “tapped” on the 17th October, and two gallons of water evacuated. After the operation he was put back upon the calomel and squills and the diuretic mixture of ol. juniperi, ol. terebinth., &c., &c. Under this treatment he improved for a week or so; but the dropsy again returned at the end of that time. In the relapse his symptoms were all aggravated, the fluid re-accumulated in the abdominal cavity most rapidly. The absorbents and various emunctories seem to have lost all power

of action. No medicine was efficient to rouse them. The patient died on the 7th November.

CASE II.—*Anasarca of the lower extremities, caused by pressure of a large tumor in the pelvic cavity.*

Esther; a negro woman; aged 50; admitted October 16th, 1860.

About a year ago this woman's right leg became very much enlarged by an effusion of serum in the cellular tissue. Under an alterative course of treatment it disappeared entirely in two months. After the lapse of eight months it again returned, affecting this time the left leg also, and in a short time the effusion began to collect in the abdominal cavity. During most of this time she was walking about attending to her duties as cook

When admitted, both legs were greatly distended, and "pitted" under pressure, and there was a considerable amount of effusion in the abdomen. Her bowels were constipated. Appetite bad, and complained of pain in the stomach. Upon examination "per vaginam," there was found contained in the pelvis a large tumor, which almost entirely filled it. It could be distinctly felt, low down in the pelvis, occupying the place of the "os uteri," which it had pushed out of its normal position and beyond the reach of the finger. It could be felt through the abdominal walls. Judging from the position and mobility of the tumor, it was in all probability connected with both the uterus and the pelvic parietes. She was kept quietly in bed and freely purged with potass. bitart. and pulv. jalap. The infusion of digitalis was also given. Under this treatment the effusion had almost disappeared by the 28th October. She was then put upon syr. ferri iod. and tinct. cinchoni. comp., her bowels being at the same time kept regularly opened with potass. bitart. and jalap. Under this treatment she has improved still more. Although there is little or no swelling about her legs so long as she remains perfectly quiet, yet whenever she is allowed to walk about much, it re-appears.

CASE III.—*Chronic Diarrhœa accompanied by Anasarca of the Lower Extremities.*

James; a negro man; aged 45; admitted Oct. 22, 1860.

This man has been hired at a brick-yard during the past year, where he was obliged to work continually in mud and water. Last July he was attacked with diarrhœa, which has continued up to this time in spite of efforts made to check it. After he had had diarrhœa about two months, he observed, for the first time, that his feet and legs were swollen.

He has now eight or ten large watery discharges from his bowels daily. His lower extremities are a good deal distended by dropsical effusion, there is also some puffiness about the eye-lids. Tongue pale and moist. Pulse slow and small, and appetite quite good. *Ordered*, R.—Hydrarg. chlor. mit. gr. ij.; pulv. Doveri, gr. iij. *M. et sig.*—Ter in die. Diet—milk and rice.

Oct. 27: Very much improved. Swelling about the feet and legs has disappeared. Bowels not move oftener than twice daily. Mouth a little sore. Stop all treatment.

Oct. 31: Has not had an operation from the bowels for two days. Feet slightly swollen again. *Ordered*, R.—Ol. ricini. ʒss; tinct. opii. gtt. xx. *M. Sig.*—Give at once.

Nov. 5: His diarrhœa has entirely disappeared. Is weak and feeble, has very little appetite. Has some swelling about his feet occasionally. *Ordered*, R.—Ferri, ferro-cyanid. gr. iv.; potass. iod. gr. v. *M. Sig.*—Ter in die.

A summary of the Clinical Remarks delivered by Prof. DAVID H. TUCKER, upon the above reported cases of Dropsy.

By dropsy, gentlemen, we understand a collection of serum in the cellular tissue or any of the cavities of the body, or in both. We generally speak of it as being active or febrile, or passive or chronic. Active or febrile dropsy generally occur suddenly—as in a person exposed to the influence of external

cold and moisture, while heated and fatigued—and in persons who expose themselves too soon while recovering from any of the exanthemata. Passive or chronic dropsies are more slow in their development, and are, for the most part, dependent upon some obstruction to the venous circulation, originating either in the veins themselves or from disease producing certain alterations in the physical or secreting character of some one or other of the organs of the body. We may have dropsy of the foot and leg, caused by a chronic inflammation of the femoral vein, blocking it up and preventing the venous return from the limb, as in the disease known as “phlegmasia-dolens” or “milk leg;” or we may have anasarca of the lower extremities caused by pressure of the gravid uterus upon the iliac veins, which disappears after delivery. Those diseases of the heart, which prevent a free circulation of the blood through that organ are the most fruitful sources of dropsy. As also diseases of the liver, spleen, kidneys and lungs. Debility is a potent agent in the production of dropsy. Chlorotic and anemic persons are frequently the subjects of dropsy; and this may be accounted for by the vitiated condition of the blood, causing not only debility and relaxation of the voluntary muscles, but also of the involuntary or muscles of organic life, thereby favoring exhalation, and also retarding the circulation. As a rule, dropsy may be considered as the symptom of a disease, and not the disease itself. Except in a few rare instances, it is always dependent upon some serious organic affection—hence idiopathic dropsy is most rare, and symptomatic dropsy most common.

The treatment used in cases of dropsy, should consist of medicines that will tend towards the removal of the cause, while at the same time it assists in carrying off the effused fluid. To remove the dropsy and hasten absorption, diuretics, diaphoretics and cathartics are used; the extent to which the latter may be carried, is regulated by the strength of the patient. Squills, calomel and digitalis is a most excellent combination and includes every indication. Our desire is to rouse the absorbents to increased action; not that we believe

they are solely at fault, because the exhalents are equally if not more so. The effusion being removed, we direct our attention more particularly to its cause, and, if possible, by proper treatment remove it also.

In case No. 1, (Joe,) the dropsy is dependent upon valvular disease of the heart, occurring in a man whose constitution has been destroyed by hard drink. We can only use palliative remedies in this case, such medicines as will remove the effusion and thereby relieve his sufferings to a certain extent, but it is impossible to cure the organic disease upon which the dropsy depends. He must grow weaker from this continual drain on the system, and sooner or later die uncured.

In case No. 2, (Esther,) the dropsy, as indicated by vaginal examination, is clearly dependent on a tumor existing in the pelvis and pressing upon the veins which return the blood from the inferior extremities. At first, when the tumor was smaller than it is now, the veins of one side was pressed upon—hence we observe the dropsy only in one limb—but as the tumor increased in size, its pressure has extended to the veins of the other side, inducing dropsy of the corresponding limb. The treatment by position, &c., has resulted in the diminution of the dropsy; but, if our theory is true, the dropsy can only permanently disappear with the removal of the tumor. A result hardly to be expected, though every effort will be made to relieve her.

In case No. 3, (James,) diarrhœa was present, and it is not uncommon to see such cases, when long continued, accompanied by dropsy—due to debility, relaxation of blood vessels, and a chemical and physical change in the blood. In this case, however, we are disposed to think the dropsy is dependent upon a more serious cause; namely, that there is ulceration of the bowels, inducing as a consequence either induration, softening or suppuration of the liver. The liver, then, by the change in its physical condition, interferes with the circulation of blood through its structure, thus inducing a form of dropsy known as hepatic dropsy.

2. LECTURE ON TYPHOID FEVER, WITH CASES.

CASE I. "*Typhoid Fever*."

Isabella; a negro girl; aged 14. Admitted October 17th, 1860.

About ten days ago, this girl was attacked with a chill, which was followed by continued fever, pain in the head, furred tongue, diarrhœa, &c.

October 17th: Pulse quick and weak. Skin hot, dry, and rough. Tongue covered with thick brownish-looking fur. Bowels loose—has from three to four large watery discharges daily. Pain in the right iliac region with tympanitis. Has no appetite. *Ordered*, R.—Quiniæ sulph. gr. ii.; spts. æther. nit. gtt. xx. M. et sig.—Ter in die. Tinct. opii, gtt. xv., and repeat every five hours, until the purging ceases. Eight ounces of milk toddy per diem. Give a little strong beef soup.

October 22d: No better. Pulse 110, and weak. Eyes injected. Pain in the head and over the right iliac region. Tongue dry and brown. Diarrhœa stopped. Scarcely eats anything. Lies in a stupid condition; can only be made to answer by speaking sharply to her. *Ordered*, Continue the same treatment.

October 25th: Little better. Pulse 84, and stronger. Skin cooler and more pleasant. Tongue cleaner and moist. Appetite improving. Bowels constipated; have not been moved for four days. *Ordered*, R.—Pilul. hydrarg. gr. v. Sig.—To be taken at bed-time. Stop the milk toddy.

October 30th: Improving slowly. Skin almost natural. Bowels moved about once in forty-eight hours. The pain in the head and abdomen has quite disappeared. *Ordered*, Stop all previous treatment, and give tinct. cinchon. comp. ʒi., three times daily.

November 4th: Convalescent.

CASE II. "*Typhoid Fever.*"

William; a negro boy; aged 15. Admitted October 18th, 1860.

Was first taken sick with a chill, about six days ago, and has not felt well since.

He has now slight fever. Furred tongue. Constipated bowels. Dull, sleepy expression. Abdomen free from pain. Says he does not feel sick anywhere particularly; but feels very tired and sleepy. Appetite tolerably good. *Ordered*, R.—Quiniæ sulph. gr. ij.; spts. æther. nit. gtt. xx. M. et sig.—Ter in die.

October 19th: Seems to be better to-day. Has very little fever. No pain about the abdomen, nor in any other part of the body. Bowels moved once during the night. Continue the same treatment. Give strong soup and rice.

October 23d: No change in his condition. Says he wants to get out of bed and walk about. He has a good appetite; is not emaciated; and, without a careful examination, one would be apt to think him in perfect health.

October 25th: Worse. Pulse 108. Bowels constipated; have not been moved for several days. Tongue moist and thickly covered with white fur. Has some slight soreness felt for the first time over the right iliac region. *Ordered*, R.—Pilul. hydrarg. gr. v. Sig.—To be taken at bed-time, and continue the previous treatment.

October 26th: Bowels moved three times; stools dark-colored and offensive. Pulse still rapid; beats 100. Tongue a little dry and brownish. *Ordered*, Continue the quinine.

November 1st: Better. Sat up for a few minutes to-day; soon became fatigued. Has had no motion from the bowels since the blue mass, given the 25th inst., ceased operating. Bowels have been obstinately constipated throughout the whole period of the fever. Abdomen slightly tympanitic; no pain or soreness. *Ordered*, R.—Pilul. hydrarg. gr. v. Sig.—To be taken at bed-time.

November 8th: He has gradually improved since the 1st inst. Pulse is more quiet; 87 to the minute. Tongue cleaner. Bowels moved once daily. Sit up a few hours during the day. *Ordered*, Stop the quinine and spts. æther. nit. and give tinct. cinchon. comp. ʒj. three times daily.

A summary of the Clinical Remarks delivered upon the above Cases, by Professor TUCKER.

Typhoid fever is a form of continued fever, and in the present state of our knowledge is an idiopathic fever; for it is probable the peculiar poison upon which it depends acts primarily upon the blood, and that the lesions of the solid viscera, found after death accompanying this disease, are results and consequences of the morbid alterations of the fluids in the body. It is true that this alteration cannot be possibly proved; but it is equally certain that the solid alterations only occur after the disease has existed in the system for a longer or shorter time. To sustain this probable theory the following illustration may be employed: The essence of tubercles must exist in the blood, although it cannot be detected, and it can only be recognized when, under the law of tubercular development, it is found deposited in the pulmonary structure or elsewhere. So the essence of typhoid fever cannot be detected in the blood; but is, under the law which governs typhoid fever, found deposited in the glands of Peyer, giving rise to enlargements, softening and ulceration of those glands at a more or less advanced stage of the disease. It is possible that this is the mode which nature adopts for eliminating tubercular and typhoid matter from the system. At one time this disease was confined to certain localities; but of late years it has become widely spread throughout the country, taking the place of other fevers, and impressing its asthenic type or "livery" upon almost all diseases. Its duration is extremely uncertain—convalescence generally occurs after the third week, though, in some instances, the fever continues to the fifth and sixth week, and sometimes even longer.

There is met with constantly every possible grade, between the mildest and severest forms of this disease. In the milder forms, we have scarcely any fever; no diarrhœa; no pain in the abdomen; tongue slightly furred, and the nervous system unaffected; the patient is unwilling to be confined to bed, though he feels too unwell and weary to stir about. In the severer forms, we have a quick and feeble pulse; diarrhœa; pain in the abdomen; tympanitis; a dry tongue, thickly covered with a brownish coat, and delirium or somnolence. This form of typhoid fever is apt to be accompanied by grave lesions in some of the various organs of the body, which are always serious complications, and render the case exceedingly unfavorable, though the mildest form may sometimes prove fatal from the same cause. This is one of those diseases that possesses that self-limiting power observed in eruptive fevers. Medicine has no power in arresting it, though rational treatment possesses considerable influence in modifying the intensity of the disease, and arresting its fatal result. The expectant plan of treatment is the one I have adopted and generally found most efficient. Nurse your patient well, and sustain him with good diet and tonics, or stimulants, if necessary, and meet the various complications, as they occur, with appropriate remedies.

In these two cases, we have the girl with severe headache, pain in the abdomen, diarrhœa, and loss of appetite, while the boy has been almost entirely free from any such symptoms, and has been obstinately constipated. His bowels had to be moved by the exhibition of a mild cathartic several times during his sickness; each time his stools were much larger and more frequent than would be supposed from the dose exhibited, showing the irritability of the intestinal canal and strong tendency to diarrhœa in this affection.

The girl was convalescent by the third week; the boy not until after the fourth, although the girl was the sicker.

ART. II.—*Sarco-Cirsocele*. Operation. Reported by B. ROEMER, A. M., M., D., Otter Bridge, Va.

I GIVE the following case, from notes made at the time at which the patient was in charge, to which I will add a few remarks, which, I hope, may not be deemed superfluous.

Oct. 27, 1857: Mr. R.; æt. 28; by profession a carpenter; applied for treatment. Much emaciated, face flushed, eye wandering and winking. Weighs about 140 pounds; his frame strong; weighed, before his present disease reduced him, from 180 to 200 pounds. Relates that in the fall of 1849, he was thrown from a horse, its left forefoot resting for some time upon the groin and scrotum. Suffered since some pain up to 1851, when epididymitis supervened. Felt since January, 1855, increased pain in the scrotum, back, and left testicle, caused, as he thinks, by too active employment. His habits chaste and temperate; non-syphilitic. The scrotum elongated since 1855; left testicle softening and wasting away. His mind became considerably weakened; is unable to express himself with precision and forgets at times to execute a contemplated work. When examined, (1857,) he presented the following symptoms: Scrotum reaches the knee; filled with varicose veins of the spermatic cord; left testicle soft; somewhat smaller than the right; painful; sympathetic pain up in the side and down the thigh; mind unsettled; hypochondriac. My diagnosis resulted in cirsocele; perhaps combined with a structural disease of the left testicle. Had various physicians attending him, without "principiis absta." The palliative treatment consisted in the use of a truss, resembling that of Evans', aconitum, local applications of iodine, etc.

Nov. 29: Saw Mr. R. again to-day. No improvement; pains perhaps less severe. Recommended Maunvir's proceeding for ligation of the spermatic vessels, with sufficient alteration in order to investigate the condition of the testicle. The patient requesting an operation, I continued former treatment, expert. gratia, until December 24th, on which day I was met by Dr. T.

Kabler, of Campbell. Bowels and bladder being emptied, I confined the patient in the usual manner, and enucleated the upper third of the cord by an incision, two and a half inches in length. The cord here dilated one half inch in diameter. Prolonging this incision to the more depending part of the scrotum, I found the testis adhering to the septum, the spermatic vessels of still greater distension and the epididymis much enlarged, resembling the *caro adnata ad vasa*. This enlargement was greatest at the reflection with the *tunica vaginalis*. The dartos in its septal vicinity healthy, but the externo-scrotal membranes were of a purple hue and partly disorganized; engorgement considerable. The posterior and upper portion of the testicle has the same color, deepening to a centre, and giving here an irregular protuberance. Blood vessels enlarged. On puncturing the centre above described, a purulent and foetid matter was discharged. Acquainting the patient with his real condition, I tied the cord (the testicle having been fully freed) at its upper third, (interrupt. ligatures,) and removed the diseased portions. The wound was dressed in the usual manner, leaving a tented opening at the most depending part. Secondary hemorrhage followed in four hours, amounting up to the time of my hasty call to the patient, to one-half gallon. Local applications proving of no effect, I removed the dressing and took up a small dartal vessel, which by a quasi muscular contraction had escaped previous attention. The œdema of the scrotum and penis was successfully overcome, and the upper portions of the incision united by first intention in the course of one week. The ligatures were removed on January 17, 1858, and the patient discharged.

Remarks.—Cirsocoele represents a varicose condition or distension of the spermatic veins, with or without a subsequent atrophy of the testicle, but always unaccompanied with any local and structural disease of the testis. Sarsocoele consists in a structural disease of the testicle, rendering it sometimes, as the name implies, in appearance like a fleshy tumor, at other times mainly giving locality to scirrhus, cancerous or tubercular, etc. ulcers, the effects of which generally tend to enlarge

the testis, and which may or may not cause a varicose condition of the spermatic cord in its continuity, though in the course of time the primary disease not being arrested, it will always transplant itself upon the testicular vicinity of the cord by contiguity. The case here given had the characteristic evidences of both, hence my choice of names.

The dissection of the gland revealed a tumor, ranging towards the centre of the testis. Under the microscope no tubuli could be detected, but many compound cells of a cancerous nature.

The term sarcocoele, in its generic meaning, is by Hastings pre-supposed in his "Abscess of the Testis," "Scrofulous Testicle," and "Tumors of the Testes." Wm. Gibson seems to apply it to all structural disease of the testis, except those of a scirrhus or cancerous nature; and Druitt gives chronic inflammation of the testes as synonymous with sarcocoele. The causes and the progress of the disease are such as to leave much in doubt, especially in reference to exact facts of a primary affection, and it is only by analogy that I place the beginning of the present case in the direct injury received from a continued pressure upon the testicle, years before varicocele was discovered. While the partial atrophy of the testicle was owing to the varicose condition of the spermatic veins, the testicular disease was yet the first and final affection, bearing to the other symptoms of the case the relation of cause and effect. Retrograde cirsocele stops where sarcocoele commences, i. e., in its proportionable effects upon the structure and the economy of the testis.

The clinical reports of the principal operations performed in the London Hospitals have given us the results of a number of extirpations of testicles, an abstract of which I subjoin.

During December, 1854, and May, 1855, thirteen cases presented themselves, of which—Died, 5
Recovered, 4
Doubtful, 4

The nature of the disease in the first five was—

Cancerous 2,	}	Died from subsequent transference of the
Scirrhus 3,		

disease upon the cord in its course.

In the four recoveries—

1 Cancerous.

3 Scrofulous.

And in the (as yet) doubtful—

1 Fungous.

1 Scirrhus.

2 Medull. Cancerous.

The ratio of recoveries 30 1-12, and of deaths $38\frac{1}{2}$ per cent.

The Hosp. des Invalids at Paris gives from August, 1855, to September, 1856, eight cases, of which—

1 Died, fibrous.

4 Recovered, cystic sarcoma, cystic, cyst in tunic. vag., and tubercular.

3 Doubtful, medull. cancerous, cystic and cancerous, and strumous—

A ratio of 50 per cent. recoveries, and $12\frac{1}{2}$ per cent. deaths.

From the history of the cases in both instances, we ascribe the greater per centage of deaths in the London Hospitals to the strict extirpation of the gland, instead of sacrificing the whole scrotal cord in order to obviate a re-location of the attacked disease.

Considering the difficulties which beset the most humane surgeon from all sides in his attempts to pronounce an ulcer malignant or non-malignant, especially if seated in the testes, (when a post mortem, or at any rate a post operationem, only can satisfy him,) it becomes necessary to decide on extirpation, only from the combined results of judging the disease per se, and of weighing the probable effects of the disease upon the general welfare of the patient from and by those effects already witnessed. The naked or armed eye is here not sufficient.

Nov. 1860: Mr. R. has called frequently at my request, and I can now fully confirm his perfect restoration to health. *Dixit se coitum fecisse et semper emissionem seminis, qua de causa nuptialem diem non longe abesse affirmavit.* Mr. R. has regained his usual weight, and is free from depression of spirit. Has been actively engaged in his profession since the spring of 1859.

ART. III.—*Ligation of the Posterior Tibial Artery.* By
THOMAS L. HUNTER, JR., M. D.

ON the 6th of August, whilst a party of school-boys were at play, one of them threw a knife with considerable force, striking the right foot of a lad, named John T., aged 14, the point of the blade entering just below the internal malleolus, making a very slight wound in the integuments, not over an eighth of an inch in length, and puncturing the posterior tibial artery at its entrance into the sinuosity of the “os calcis.” The hemorrhage, which was considerable, was controlled by the school-master, who applied a ligature tightly around the leg.

The boy was carried home, where I, in the absence of my father, Dr. Hunter, was called to see him about five hours after the accident occurred.

Upon removing the ligature, the hemorrhage was not renewed, though the pulsations of the artery could be distinctly seen under the external wound. Judging from the position of the vessel—lying upon the bone and thinly covered with fascia and cellular tissue—that it might be easily compressed, and thereby obliterated, without the necessity of deligation, I applied a tight bandage from the toes, carrying it above the ankle, and secured a compress firmly over the wound; and directed the patient to be kept perfectly quiet in bed for some days.

Aug. 9th: Examined the foot; found the wound in the integuments had healed by the first intention; parts around slightly swollen; pulsation in the artery stronger and more apparent than natural. Re-apply compress, and bandage tightly.

Aug. 13th: Quite unwell to-day. Had a chill yesterday, followed by high fever, which lasted all night, and has not entirely subsided this morning. Tongue furred. Examined the foot and found a false aneurism had formed about as large in circumference as a half dollar. The leg was inflamed and

swollen for some distance up. Complained of much pain about the foot. Re-applied the bandage, placing the compress a little above the tumor. Gave pilul. hydrarg. gr. viij; quiniæ sulp. gr. iv; every two hours, until he had taken gr. xij; and applied a wet cloth to the leg.

Aug. 14th: Saw him this morning. His general condition better. No fever. The aneurismal tumor was enlarged and the integuments covering it very much stretched and thinned. The pulsation and thrill was very distinct. Fearing that the recent adhesion in the external wound would soon give way, I left the house, intending to return as soon as I could procure an assistant, and tie the posterior tibial artery above the wound. I had scarcely left the patient two hours before I was hastily summoned to him, with the notice that he was "bleeding to death." Upon arriving at the house, I found him in almost a dying condition. The tumor had burst and a large amount of blood had been lost. The pulse at the wrist was scarcely perceptible, and the surface cold and moist. I remained with him the rest of the day and through the night, giving stimulants and controlling the bleeding by means of a tourniquet applied to the popliteal space and a compress tightly tied over the track of the artery just above the wound; the pressure alternating between the two. The following morning at nine o'clock, having secured the assistance of Dr. F. F. Nine and Dr. Thomas L. Hunter, I cut down upon and succeeded in tying the posterior tibial artery in its lower third. The leg was so much swollen as to conceal, to a great extent, the natural relation of the parts; and in addition to the above complications the usual guide to the artery, its pulsation, could not be had, on account of the pressure on the vessel by the tourniquet above, which was necessary to be kept up for fear the patient might lose more blood—his condition being such as not to admit of the loss of another ounce.

An attempt was made to administer chloroform, previous to operating, but it caused so much nausea and vomiting that it had to be stopped. An incision was made, commencing about four inches above the internal malleolus and about half an inch

from the internal border or angle of the tibia, running obliquely downwards and backwards to within an inch of the malleolus. After cutting through the integuments and cellular tissue, the superficial fascia and intermuscular aponeurosis were divided upon a grooved director, and the sheath of the vessels exposed; this was also carefully opened, upon a director, and the artery isolated and tied. The wound was then brought together by adhesive strips and the foot covered with cotton and lightly bandaged.

Five days have elapsed since the artery was ligated, and the patient has in that time steadily improved. His general condition is better. There is not the slightest pulsation in the old aneurismal tumor, and the tumor itself is gradually being removed. The wound necessarily made in ligating the artery, has closed, with the exception of a small portion, out of which lies the ligature. The patient is in a fair way to be well in a short time, unless some accident occurs.

The swollen condition of the leg and the extreme debility of the patient greatly enhanced the difficulties of the operation. The attempt to use chloroform so reduced the already feeble circulation, that the pulsation of the artery was scarcely perceptible after it had been exposed, although the pressure of the tourniquet had previously been removed; and it was only possible to distinguish the pulsation, by introducing the end of a probe under it, and allowing the other end to hang free; there could then be observed a motion in the free extremity of the probe, synchronous with that of the heart. It was impossible to perceive any pulsation by the touch.

Mr. Guthrie, I think, insists that an artery, when wounded, should be tied at the place of the injury. Other surgeons of high reputation, advocate the deligation of the artery at another place, always of course on the cardiac side of the wound. It is evident that the recommendation of Mr. Guthrie must be carried out at the time of the injury, or soon after. It seems to me that, taking into consideration the favorable situation of the wounded artery, it was good surgery to attempt the cure in this case by compression instead of by ligation.

It proves, however, that an artery of any size, when wounded, is not to be obliterated in that way.

It may also be remarked, that this case exemplifies some of the delectable embarrassments and difficulties that occur in the professional career of a country physician. It is manifest that the operation ought to have been performed on the preceding day, when the aneurismal tumor burst, but how could a young tyro in surgery do a rather difficult operation, alone and unassisted, and without the aid and comfort of having brother physicians to share the responsibility?

ART. IV.—*Contributions to Urology.* By WILLIAM A. HAMMOND, M. D., Professor of Anatomy and Physiology in the University of Maryland, &c.

It is a principle indissolubly connected with vitality in both plants and animals, that decomposition of the tissues constituting their several structures is incessantly taking place, either as a cause or consequence of the various organic actions they are called upon to perform. The products of this constant disintegration are incapable of entering into such combinations as the healthy organism requires for its nutrition, and although, under certain abnormal circumstances, some of them do contribute to the maintenance of functions necessary to life, they must, nevertheless, be regarded as essentially effete. Accordingly, we find that they are continually being removed to make way for newer and more vigorous material, and after in many instances undergoing further degradation, eventually fall to the inorganic class of substances whence they originally came.

With tissue-destruction, as it occurs in the vegetable kingdom of organic nature, it does not come within the scope of this memoir to treat; but a few of the main facts connected with its course in animals may very properly engage a portion of our attention.

It may be considered a fully established law, that the decay of the tissues of any organ of the system is in direct proportion to the functional activity of that organ. By the term decay, it is not intended to refer to atrophy or degeneration, which are inversely to the functional activity of a part, but to that interstitial death of tissues which always, in health, calls for increased deposition of new material, and may even produce hypertrophy.

We should, *à priori*, expect the existence of the law above enunciated; for, even in the operation of artificial machines, we find that those parts involved in the greatest degree of action are soonest worn out. In the galvanic battery, for instance, the amount of dynamic electricity evolved is always in proportion to the quantity of matter subjected to chemical action, and, consequently, we are enabled to measure the *relative* amount of the one from the *absolute* amount of products resulting from the operation of the other. It must not be forgotten, however, that an essential distinction exists—in addition to the radical differences of composition and structure—between the parts of such machines and those of an organic being, in this, that in the latter there is a compensating deposit of new substances, whilst in the former no such action occurs.

Experiments are not, however, wanting to support the principle above stated. Increased muscular exertion—*cæteris paribus*—is invariably followed by an augmentation of the quantity of urea and other constituents of the urinary excretion, as has been determined by the researches of Simon,¹ Lehmann,² myself,³ and others.⁴ Excessive mental exertion, as is well known, induces an increased elimination of phosphoric acid through the kidneys, and some time since I attempted to show the relation existing between the amount of this substance excreted and the extent of mental exercise.

¹ Animal Chemistry, Cavendish Society's edition, vol. 2, p. 168.

² Physiological Chemistry, Cavendish Society's edition, vol. 1, p. 163.
American Journal of the Medical Sciences, January, 1853.

⁴ North American Medico-Chirurgical Review, vol. 1, p. 730.

Rummela⁵ and Draper,⁶ it is true, arrive at conclusions which, so far as they relate to the increased elimination of urea as an effect of augmented physical exertion, are at variance with the above; but the great mass of authority is in accordance with the view above given.

It has also been ascertained, that the excretions from the skin and lungs are augmented by increased wear of these parts of the organism. Numerous authorities might be adduced in support of this statement; but it is a fact so patent to every observer, that the simple allusion to it is sufficient.

Of the several channels which exist for the removal of decomposed tissue from the system, that of the kidneys is the only one which is purely and entirely excretive. The lungs perform the double office of eliminating carbon and absorbing oxygen; the skin gives off water, chlorides, phosphates, &c., and is also capable of taking up substances placed in contact with it; and the intestines serve for the absorption of nutriment, and for giving exit to unassimilated food, as well as for the evacuation of products of destructive metamorphosis. Thus, therefore, when we place a ligature around the trachea of an animal, death ensues from two causes; when the skin of a frog is covered with a varnish impermeable to air and moisture, the animal dies as well from obstruction to absorption as from arrest of elimination; and when the function of the intestines is stopped, death ensues more from starvation than from the non-removal of effete matters from the organism; but if the renal arteries be tied, or the kidneys removed from an animal, death is essentially due to arrest of excretion, and the consequent retention in the blood of substances fatal to organic existence.

The urinary excretion, within the limits of health, is subject to great variation, both in quantity and constitution, from the operation of various disturbing causes. Thus the quantity of water may be influenced by the quality of the food, by medi-

⁵ *Verhandlung der Phys.-Med. Gesellschaft zu Würtzburg*, Band. V, L. 119.

⁶ *Draper's Treatise on Human Physiology*, New York, 1856, p. 220.

cines, and by the temperature and humidity of the atmosphere. The amount of solids dissolved in it is also subject to alteration from like factors; and, in short, any cause affecting the rate of retrogressive tissue metamorphosis, and, as a consequence, the quantity of products arising from tissue-decay, must modify, more or less, the character of the urine. When we reflect that every contraction of our muscles, every action of the various glands of our bodies, every thought of our brains—in fact, every functional operation, however short it may be—involves the destruction of a certain amount of tissue, we can well perceive how varied must be the composition of an excretion containing the representatives of this decay. It is impossible, therefore, to assign any definite composition to the urine, so far as relates to the quantity of any of its component parts.

Various analyses of the normal human urine, showing its composition in 1,000 parts, have been made. As might be expected, no two of these are alike, and, for all physiological or practical purposes, must be nearly or altogether useless. It is only by analyzing the entire quantity of urine evacuated in a given time, under known conditions of health, food, physical and mental exercise, &c., that any indications useful to science are likely to be obtained.

Great diversity exists in the composition and character of the urine as excreted by the several orders and classes of animals. In the invertebrates, we know but little of this fluid. In the excrements of certain insects, uric acid and urates have been discovered. I have found very beautiful crystals of uric acid in the excremental matters of certain larval sphingidæ, and also in those voided by several species of coleopterous insects.

In fish, the urine has been but little studied. Davy¹ found that a nitrogenous compound—either urea, a combination of uric acid, or some other azotized substance—was excreted. Jones,² in the urine of the bass (*corrina ocellata*), detected uric

¹ Transactions of the Royal Society of Edinburgh, vol. xxi, p. 543.

² American Journal of the Medical Sciences, No. lviii. April, 1855, p. 334.

acid, oxalate of lime, phosphates, and chloride of sodium; and in that of the white fish (*corrigonus alba*), I have found all these substances, together with urea and sulphuric acid.

In reptiles, we meet with considerable diversity in the composition of the urine. In frogs, it contains urea in quite a large proportion, chloride of sodium, and phosphoric acid. In serpents, it is composed for the most part of urates, although under certain circumstances I have detected urea. In lizards, both urea and uric acid are found, together with phosphoric acid in small amount. In the urine of *plectiodon obsoletum*, the urea amounted to 25.00 of the total quantity of solids, and in that of *ophisaurus ventralis*, to nearly 15.00. In *phrynozoma cornutum*, there was barely a trace of urea, but uric acid in large amount. In the chelonians—the urine of which I have examined—both urea and uric acid were found, together with hippuric, sulphuric, and phosphoric acids.

In birds, the urine consists almost entirely of uric acid in combination with ammonia and lime. In common with almost every other observer, I have never detected urea in the urine of this order of animals.

In the mammalia we meet with a more complex excretion. In the carnivora, however, it consists mainly of a solution of urea in water. The urine of a cat weighing $4\frac{3}{4}$ pounds, fed for several days on a diet composed exclusively of animal food, I found to consist as the average of five daily analyses of

Water,	90.32 cubic centimetres
Urea,	9.50 grammes.
Inorganic salts,	1.47 grammes.

The urine of the herbivora is a far different fluid. Instead of being acid, it is always alkaline, contains no uric acid, but abounds in alkaline carbonates, and other substances not present in the urine of the animals last mentioned.

The urine of man is an exceedingly complex fluid. Being the most highly organized of all animals, it is reasonable to suppose that this would be the case; for the more simple the structure of an organism, the more simple in composition are

its excretions, and as organization becomes more complicated as we ascend the scale of creation, the excretions, made up as they are, mainly of the detritus of organs, come to contain substances not present in those of less elaborately constructed beings.

The main purpose of the kidneys is, undoubtedly, to remove the nitrogenous products of tissue metamorphosis from the system. Such substances, we find, especially in man, constitute a large proportion of the solid matters of the urine, amounting in many instances to more than 75.00 of the total quantity eliminated in the twenty-four hours. In addition, there are various substances belonging to the mineral kingdom.

The primary division of the constituents of the urine, would, therefore, be into the organic, and inorganic.

Under the head of organic substances are embraced:

1. Urea.
2. Uric acid free and in combination with lime, soda, magnesia and ammonia.
3. Hippuric acid in combination with soda and perhaps other bases.
4. Creatine.
5. Creatinine.
6. Oxalic acid combined with lime.
7. Coloring matters.

The inorganic constituents of the normal human urine are as follows:

1. Water.
2. Chlorine combined with sodium, potassium and ammonium.
3. Sulphuric acid in combination with lime and potash.
4. Phosphoric acid in combination with lime, magnesia, potash, soda and ammonia.
5. Carbonic acid combined with lime.

The average amount of urine excreted in the twenty-four hours by adults in good health, taking a moderate amount of

exercise, and eating a due proportion of animal and vegetable food, I have found, as the result of numerous determinations, to amount to from 900 to 1600 c. cm. The solids in the same period, and under like conditions, ranged from 30 to 110 grammes. In ten healthy adult males, weighing each about 165 pounds, being from 23 to 25 years of age, and living under similar conditions of food, exercise, &c., I obtained as the average of ten examinations in each individual case, the following:

Whole quantity of urine, 1102.11 cubic centimetres.

Solids, 55.46 grammes.

We see, therefore, how great is the amount of drainage from the system through this channel, and we can readily perceive how important it is, in a sanitary point of view, that it should continue uninterrupted.

The modifications which the urine undergoes, in health and disease, I purpose considering in a future paper.

ORIGINAL TRANSLATIONS.

- I. (*The Pathogenesis of Chlorosis.*) (From a Report on the Secretion of Sugar in the Liver, made in the Society of Physicians at Kiel.) (Translated from the "Archiv der Wissenschaftlichen Heilkunde," vol. iv, part 3.) Göttingen: 1860.

It is known that fewer colored blood-corpuscles are found in chlorotic than in healthy blood: while in the latter one cubic millimeter contains from four-and-a-half to five millions, the

number in the former falls as low as to two-and-a-half millions in the same quantity. It is known, too, that the colored blood-corpuscles contain iron, and that chlorotic blood is therefore deficient in iron; and further that the deficiency of chlorotic blood in colored blood-corpuscles (and in iron) is the consequence of an impaired state of the formative functions (Anamorphosis), and not the result of increased waste (Kata-morphosis); for the urine of chlorotic patients is poor in solid materials. Lastly, it is known that the small amount of iron which the healthy organism appropriates to itself from the most varied diet—as from flesh, milk, eggs, water, &c., fully suffices for the needs of the system; especially as in the healthy state the bile is the only secretion that contains iron, and yet the ferruginous contents of the bile are in great measure re-absorbed from the alimentary canal. How, then, is the occurrence of chlorosis in a healthy girl to be explained? She receives still the same ferruginous articles of food; it cannot then depend upon a want of iron, or upon a withdrawal of iron from the system, for these conditions do not exist; and yet the girl, with a constant supply of the same amount of iron, becomes chlorotic, i. e. there is a failure in the formation of red blood-corpuscles. What is this owing to? The vitalist, who ascribes to the sanative power of nature a Prometheus-like contrivance and action in the preservation and restoration of health, is compelled to have recourse to a supposed error or caprice in regard to the direction that this sanative power takes, in order to explain the occurrence of chlorosis. But I am glad that the time is past in which such phrases and terms are deemed satisfactory, and in which it was fancied that an already obscure subject was to be explained by something utterly unintelligible. Now when we ask for a substantial reason as to why chlorosis should occur with a sufficient supply of iron, the theory hitherto held is at a loss for an answer. The theory, to be sure, contains the truth, but it does not contain the whole truth; a link is wanting in the account of the origin of the disease. The organism in fact lacks the power to apply the iron furnished it to the formation of hematine. Upon

what does this want of power depend? We find the answer to this question in a discovery of Lehmann. Physiological chemistry has, up to this time, made the various excretions the objects of its researches, and especially the urine. Its results, therefore, at best, have been of interest only as means of diagnosis. But where it has more thoroughly investigated the changes of tissue, fruitful results are to be found for practice, for pathology, and for therapeutics. I may mention, for example, the value of that beautiful discovery of Halwachs and Kühne, that benzoic acid in its passage through the liver is converted into hippuric acid by the decomposition of the glycolic acid. From this Falck inferred, that benzoic acid must be almost a specific against the condition known as icterus; and experience has proved it to be so. In like manner, for the explanation of the origin of chlorosis, I will show the value of Lehmann's discovery that hematine is a glucoside.

We know from Bernard that the liver is a sugar-secreting organ; and we know, also, that in disease the secretions of the different glands vary in amount, appearing at one time excessively increased, and at another diminished or even entirely suppressed. There is no conceivable reason why this should not be the case with the secretion of sugar in the liver; indeed, we know already that this secretion is increased in many forms of diabetes mellitus, and that in all febrile diseases it is entirely suspended. We will now suppose, for a moment, and the supposition does not stand at all in our way, that the sugar-secretion of the liver has for sometime been diminished; what will be the first consequence of such diminution? Inasmuch as hematine requires sugar for its formation, (for, according to Lehmann's beautiful discovery, it is like salicin, phloridzin, tannin, &c., a combination of sugar, or a glucoside,) therefore, when there is a failure in the supply of liver-sugar, the formation of the coloring matter of the blood will not be accomplished, even when the amount of iron is sufficient as before; and consequently the construction of colored blood-corpuscles will be stopped; or, in other words, the chlorotic condition will originate. The essential cause, then, of the occurrence of

chlorosis is a deficiency or cessation of the secretion of liver-sugar; the fact that the supply of iron is not used in forming hematine, is only a consequence of the former circumstance, and is not the real cause of the disease.

If the supposition thus made be true, viz., that chlorosis depends upon a defective secretion of sugar by the liver—a supposition the correctness of which has, we think, been proved analytically and synthetically—three inferences may be drawn from it:

1st. Chlorosis is to be cured by means of sugar, which supplies what is wanting through the failure of the liver.

2d. Chlorosis is to be treated by every means which can restore the sugar-making function of the liver to the normal condition.

3d. The large doses of the preparations of iron with which chlorosis is empirically treated, effect the cure, not, as is universally believed, by supplying the requisite iron to the body, but because these large doses operate by promoting and increasing the secretion of sugar in the liver.

1. If the deficiency of liver-sugar is to be supplied by the ingestion of sugar, it must be a sugar like that of the liver, i. e., grape, and not cane sugar. For though the healthy organism may be able to convert the cane sugar into grape sugar, yet the question is, whether the impaired digestion of chlorotic patients is equal to the task. Does grape-sugar then cure chlorosis? In northern Sleswick, where I practiced medicine for twelve years, and as I have been told, in many parts of Hanover, honey is a popular remedy for chlorosis; and I can attest its efficacy from my own experience. Even though the honey may contain a small portion of iron, yet this is not the curative agent, for other articles of diet which contain just as much iron, are entirely powerless. As honey, by long continued use, in large doses, may produce flatulence, acidity, colicky pains and diarrhœa, it may be well to combine it with suitable correctives, as the bitters, carminatives, &c., and to take it fasting in the morning in the dose of a table-spoonful.

2. We are still entirely in the dark as to the means which

increase or diminish the secretion of liver-sugar. There is here a wide field open for enquiry into the powers of remedial agents. But, little as we know with regard to this subject, we are yet acquainted with one agent which promotes the secretion of sugar, and we find it efficacious in the treatment of chlorosis: it is nothing else than cold water. It was shown some years ago, by Dr. Petters, in the "*Prager Vierteljahrsschrift*," that the secretion of sugar in diabetes is increased by drinking cold water copiously; and the experience of every hydropathic institution proves that chlorosis may be cured by the same means.

Many physicians regard the free use of cold water as a means which acts only by powerfully increasing the waste of tissue; and therefore they give no credit to the assurances of hydropathic physicians, that chlorosis is cured by this means; for, according to their theoretic views, it must aggravate the disease.

In chemical processes, water at one time plays the part of an acid, at another that of a base; it has, also, a two-fold action as a remedy. Acting in one way, it greatly increases the waste of tissue [Katamorphosis]; acting in another, it promotes its formation [Anamorphosis]. Indeed, a glance at the development of the foetus throws light on the action of water in the organism, and may remove some rusty prejudices. Schlossberger has shown that in the earliest condition of foetal life, the blood is of all the parts poorest in water, while after birth it is richest. Since then the younger the foetus is, its vegetative life is the more energetic, and the formation of tissue is more active than its waste, it is evident that this formation is increased in activity by a large amount of water.

3. Whether the last of the three inferences will be verified, the future will show. What is chiefly needed in the enquiry is an accurate method of investigation, in order to measure exactly the variations observed in the secretion of liver-sugar after the employment of different agents. When such a method is found, then the question as to the effect of iron will be easily decided.

These remarks are not yet sufficient to prove beyond all doubt that a failure in the secretion of liver-sugar is the immediate cause of the disease; yet I think I have shown the insufficiency of the prevailing opinions on the subject to explain the morbid process, and on the other hand the high probability of the new theory. The future may give sentence in the matter. Should every doubt be finally removed, it would then be shown that chlorosis, and diabetes mellitus are in their essential nature diametrically opposite morbid processes; and experimental pathology might one day succeed in producing chlorosis artificially, as has been done in the case of diabetes, so that the means would thus be found for the radical cure of diabetes. —

Maryland and Virg. Med.
S. C. C.

II.—*Remarks upon Chlorosis, with special reference to its occurrence in Children.* By Dr. NONAT.

[In the following paper, translated from the "Journal des Connaissances Médicales" of October 10th, 1860, the distinction between chlorosis and anæmia, to which Becquerel has drawn attention, is discussed. It is shown that the supposed connection between chlorosis and disturbed uterine functions is unfounded; and the frequent occurrence of chlorosis in children is maintained.]

In this work the author proposes to answer the following questions. 1st. What is chlorosis? 2d. Does chlorosis differ from anæmia? 3d. What are the principal distinctive characters of the two morbid states? 4th. Are there really two varieties of chlorosis—the one idiopathic, and the other symptomatic? 5th. Is the disease confined entirely to women? 6th. Can it be the consequence of suppression or retention of the menses? 7th. Is it true that chlorosis is a malady of the age of puberty? 8th. Does it occur in children? 9th. What influence has chlorosis upon the organic development, and upon the constitution? 10th. What influence has the development of the

organism upon the chlorotic condition? 11th. Is there any specific treatment of chlorosis?

Before defining chlorosis, M. Nonat makes some remarks upon the "*force of hematosi*s." He gives this name to the resultant of those forces or functions which co-operate in the making of the blood. "The *force of hematosi*s," says M. Nonat, "is proportionate to the richness of the blood, or to the amount of blood-corpuscles; so that the proportion of blood-corpuscles is to be considered as the measure of the force of hematosi

s. This force varies in different species of animals, and in each individual species according to age, sex, and certain peculiar circumstances." Having studied the force of hematosi

s in the different animal tribes, the author adds: "In the human race this force is referable for both sexes, and for every age, to a standard from which it cannot deviate without detriment to the health; it has fixed physiological limits which it may not exceed, nor fall short of. It is stronger in men than in women; and in both sexes it increases with age, until the organism is completely developed: from that time it remains stationary, or, at least, its variations do not appear to have any marked influence exerted upon them by age.

"When the proportion of blood-corpuscles deviates much from the physiological standard, (the average being, according to the researches of Lecanu, in men 132, in women 115 parts in 1,000,) then the force of hematosi

s deviates likewise from the normal state.

"If this force is exaggerated in its action, then the corpuscles are in excess in the blood, and plethora is occasioned. If, on the other hand, the force of hematosi

s is lowered, the proportion of blood-corpuscles is diminished, and chlorosis is the consequence."

Chlorosis, then, may be defined as "a malady characterized functionally by a lowering of the force of hematosi

s, and anatomically by a diminution in the proportion of blood-corpuscles."

M. Nonat strongly opposes the opinion of those who regard chlorosis and anæmia as identical affections. He discusses

and refutes, one after another, all the arguments brought forward in support of this doctrine, which are derived chiefly from the similarity of the symptoms, lesions, causes and treatment of the affections. Between the symptoms of chlorosis and anæmia there is an apparent analogy, rather than a real resemblance.

In regard to the alteration of the blood, M. Nonat adopts the views of MM. Andral and Gavarret, that in chlorosis there is a diminution of the red-corpuscles only, the other elements remaining unchanged, while in anæmia there is a simultaneous diminution of all the component parts of the blood.

[The opinion that chlorosis arises from a diminution of the red-corpuscles *only* is in accordance with the view expressed in the paper translated from the Göttingen Journal. It is there stated, that the essential cause of the disease is a deficiency in the formation of liver-sugar, one of the constituents of the red-corpuscles, which are consequently not formed in sufficient quantity.]

M. Nonat thinks that most of the analyses—and especially those of Fœdish—in which a diminution both of the red corpuscles and of the fibrine is found, relate to the blood of anæmic, and not of chlorotic patients. They cannot then be adduced as an argument in favor of the identity of the two diseases.

The difference between chlorosis and anæmia is seen still more clearly when their respective causes are studied. Anæmia is an accident—the result either of hemorrhage, or of a profound disturbance of the nutritive functions by some poisonous influence.

In anæmia, the *force of hematosi*s remains unimpaired. Chlorosis is inherent in the constitution; it is congenital, and results from a defect in the organs charged with the function of sanguinification. Anæmia may be produced artificially in an animal by exhausting depletions; but, as M. Trousseau says, chlorosis cannot be produced at pleasure.

Chlorosis and anæmia may co-exist in the same subject. This complex state or *chloro-anæmia* is observed in persons

originally chlorotic who have been subjected to abundant losses of blood, or to grave organic diseases.

From the definition that he has given of chlorosis, and from the opinion that he has expressed as to its nature, it is evident that M. Nonat regards it as an essential, idiopathic disease. As to the chloroses called *symptomatic*, he classes them among the forms of anæmia, that is to say, among those alterations of the blood which are caused by the introduction of a toxic principle into the system.

The author espouses and defends the opinion now generally held, that chlorosis is a malady common to both sexes. He denies in the most positive manner, that chlorosis in the female can be the *consequence* of suppression or retention of the menses.

Those who have held the opposite opinion have mistaken the effect for the cause in persons already possessing the chlorotic diathesis, but in whom chlorosis has remained, so to speak, latent or unrecognized before the occurrence of the irregular menstruation. If we regard chlorosis, as M. Nonat has done, as an impoverishment of the blood, produced always and solely by a failure of the force of hematosiis, it is clear that the chlorotic condition must precede the derangement of menstruation, and that it cannot in any way be the result of it.

According to M. Nonat, chlorosis is not peculiar to the period of puberty; it is a disease incident to all ages, and, contrarily to the opinion generally entertained, it is more common in infancy than at any other period of life. The author discusses the history of chlorosis as it occurs in infants. Understood but imperfectly by Sauvages—its existence called in question by the authors of the *Compendium*—passed by in silence by most writers who have considered the diseases of early life—and referred to, rather than critically described, by M. Henri Roger in his memoir on auscultation of the head (October 11th, 1859)—chlorosis, as it occurs in children, has been for eight years the object of M. Nonat's persevering researches. He gives in his memoir a summary of his observa-

tions derived from 68 cases, which are divided in the following manner:

1st. With respect to sex:

Boys,	27
Girls,	41
					—
Total,	68

2d. With respect to age:

Under 1 year,	3
From 1 to 2 years,	17
“ 2 to 3 “	6
“ 3 to 4 “	5
“ 4 to 5 “	4
“ 5 to 6 “	6
“ 6 to 7 “	4
“ 7 to 8 “	7
“ 8 to 10 “	5
“ 10 to 15 “	11
					—
Total,	68

These figures show that chlorosis is met with in infancy—even in the earliest months of life; (since the publication of his work, M. Nonat has met with a case in a little girl five months old.) They show also that it is common to children of both sexes, but that it is more frequent in girls than in boys. It is seen too by this numerical statement, that the number of chlorotic children is very considerable; the author thinks that it is no exaggeration to state, that out of every ten children, eight are affected with chlorosis.

Chlorosis is strongly hereditary; it is by no means rare to meet with it at the same time in mother and child, and also in several or all of the children of a family. A bellows' murmur in the large vessels of the neck is always heard in chlorotic children. The disturbances of the nervous system connected

with chlorosis, so frequent after the age of puberty, are very rare in infancy. Chlorosis exercises a very deleterious influence on the regular development of the system—an influence always proportionate to the degree in which the force of hematosiis is lowered. The period of childhood in chlorotic patients is often troublesome; they are very susceptible of the action of morbid causes; their diseases present a remarkably adynamic character, and convalescence is always slow.

If chlorosis acts unfavorably upon the development of the organism, this development, on the other hand, produces a very beneficial effect upon the chlorotic patient. Sometimes, indeed, when a chlorotic child is surrounded by favorable circumstances, as it grows and becomes gradually developed, the force of hematosiis, hitherto imperfect in its action, is invigorated, and rises to the physiological standard. Thus order is restored, and the chlorosis is cured spontaneously, by the unaided resources of nature.

But if this salutary change does not take place at the time of puberty, then this state is established with great difficulty, and is interrupted by a thousand accidents, especially in young girls who become subject to all the disorders of difficult menstruation.

M. Nonat believes that there is no specific remedy for chlorosis, nor indeed any really curative treatment. In his opinion, iron is powerless to rectify the insufficient action of the force of hematosiis. This condition of the economy can be modified only by the gradual and uniform development of the system, by which a spontaneous cure is wrought. But, although iron is not a specific against chlorosis, it is at present the best palliative agent. It is well to have recourse to it, in order to aid the efforts of nature. After describing the method of treatment that he is in the habit of using with chlorotic infants, in which an important part is assigned to hygienic means, M. Nonat ends with a summary of the principal ideas enunciated in his work.

S. C. C.

III. *Difficulties in the Diagnosis of a Nervous Affection known as Intermittent Tetanus, Tetanillus, Rheumatic Spasms, &c.* A Clinical Lecture by M. TROUSSEAU. [Translated from the *Journal des Connaissances Médicales* of June 10th, 1860.]

WHETHER this disease existed in the time of our ancestors is a question which I will not dispute: if it did, it was at any rate but little known, and we find descriptions of it only in modern works. In 1834, Dr. Imbert-Gourbeyre, professor in the Preparatory School of Medicine of Clermont, attempted to collect in his inaugural thesis all the scattered fragments that had at that time been written on the strange disease that I am about to describe to you. But the facts were vague and not connected by any well-established theories, when, in 1845, M. Delpech, one of my *internes*, now a member of the faculty and physician at *la Maternité*, published a treatise on Idiopathic Muscular Spasms, containing a large number of carefully conducted observations. Finally, in 1852, Dr. Lucien Corvisart defended his thesis on *Tetanillus*.

During the years 1840, 1841 and 1842, I observed cases of this disease among nursing women in the large field of duty entrusted to me at the Necker Hospital. I then thought it right to designate it as *rheumatic spasms of nursing women*; but I afterwards found the malady among men, children, and non-puerperal women.

That you may the better understand the diagnostic symptoms of this disease, I will suppose that it assumes three forms—a mild, an intermediate, and a grave form, (these divisions, however, are purely arbitrary):

1st. The mild form occurs nearly always in women. The patient experiences tingling sensations in the hands and feet, and difficulty in moving the fingers and toes; the fingers then become rigid and approach each other, and the thumb is strongly adducted; so that the hand assumes a conical form like that of an accoucheur. This manner in which the fingers

are disposed is often sufficient of itself to indicate the nature of the malady. The spasmodic action is attended with hardness of the muscles of the arm and fore-arm, which ceases and re-appears soon. The formication and contraction of the muscles extend sometimes to one of the upper, sometimes to one of the lower extremities—sometimes to both sides and all the limbs at once. But these symptoms are attended with so little pain, and seem so trivial, that patients hardly regard them, and there are no signs of general disturbance of the constitution.

2d. In the intermediate form, besides the local disorder that I have just described, there is observed a slight febrile excitement—the duration of which is about seven or eight days. The appetite is diminished, and there is a certain feeling of *malaise*; but these general troubles are in no respect serious in their nature. The muscular contractions are more painful, and occur oftener; they invade the muscles of the face, of the globe of the eye, and of the pharynx. Sometimes the muscles of the tongue are implicated, and patients are unable to speak. The fingers are always shrivelled. By this time the friends of the patient have become alarmed.

3d. Four years ago, my honorable colleague and friend, M. Lasègue, was one day at the prefecture of police, when a young shoemaker, eighteen years of age, was brought to him, who had been attacked, as was said, by epilepsy, and was found lying on a heap of stones in front of the Hôtel-de-Ville. The patient's body was as rigid as a bar of iron, his jaws were firmly locked together, and his hand had assumed the conical form. After M. Lasègue had spoken with him, and ascertained that his disease was not epilepsy, he sent him to the Hôtel-Dieu, where he was admitted under my care. Here we witnessed the following phenomena: The patient was suddenly attacked, and fell to the ground in a state of tetanic rigidity. The muscles of the neck, breast and abdomen became stiff, and bent the body forward. In a few minutes extreme difficulty of respiration came on, followed by perfect orthopnoea of happily short duration. The patient's face was red, and his expression

excited; the lips were violet-colored, and the jugular veins distended. I witnessed many attacks of this kind, all of frightful violence, accompanied with pulmonary engorgement, and in every respect resembling tetanus. No sooner had the tetanic spasms passed off, than the young man conversed cheerfully, though he expressed himself with difficulty. He would rise, and render trifling services to his companions in the ward, and passed the time in sweeping and making beds; in a word, he led the ordinary life of the attendants upon the sick. In the intervals between the attacks he appeared to be entirely restored to health, and ate with a good appetite. When the attack first commenced, the patient generally was conscious of tingling sensations in the arms; he was still able to move his hand; but very soon it assumed the form I have described, the arm became rigid, and severe pain was felt. Numbness, formication and cramp are then the first symptoms of a paroxysm, which does not attain its greatest intensity for ten or fifteen minutes; then the spasmodic contractions extend to the upper and lower extremities, to the face, jaws and tongue.

Although a person laboring under this disease may have been free from all these symptoms for 24, 48, 72 or 100 hours, it is yet always possible to produce them. Indeed, I once discovered by accident a mode of causing these attacks at will. In 1839, at the Necker Hospital, I attended a woman who was subject to violent paroxysms of this disease. I then formed the opinion, which I still hold, that it is of a rheumatic nature. I called for a bandage, tied up the arm, and directed one of the resident pupils to open a vein by a longitudinal incision; for I prefer this mode of performing the operation. Suddenly a paroxysm occurred; and on removing the ligature, and placing it upon the other arm, or even upon one of the legs, the same result was observed. This experiment has invariably had the same effect. I thought, at first, that this artificial production of the spasms was due to the momentary venous congestion; but on compressing the artery so that the quantity of blood in the arm was not increased, I observed the

same effect. The cause, then, you will tell me, is the transient interruption of the arterial circulation; not at all; the action is confined entirely to the nervous cords. In some patients, pressure upon the clavicle on a portion of the nerves of the brachial plexus suffices to produce the spasmodic action. The malady may thus be recognized at any time by compressing the origin of the nerves that are distributed to a limb; and so you see we have an important means of diagnosing the disease. The spasm comes on suddenly, acquires speedily great violence, reaches its greatest intensity, and then begins to decrease. The duration of the disease varies from 8 or 10 days to 2 or 3 months; but in the latter case it is in the latent form, i. e. it may be produced by the influence of pressure.

Among the causes to be mentioned, I must tell you that an obstinate diarrhœa appears to predispose to the disease. Thus, during the epidemic of 1854, a large number of cases was observed. The boy whose case I have described to you, had no previous diarrhœa, but he had been exposed to cold and rain; and atmospheric impressions are frequent causes of the disease. The most active and frequent of all causes, however, are the puerperal state and lactation. I have here twelve beds reserved for lying-in women, and in them I meet with an infinitely larger number of these cases than in the fifty others. Why is it that there are such important circumstances in the etiology of the disease? Do not ask me. I know nothing of the reason—nothing at all. The disease, if left to itself, appears to be unattended with danger. The functions of organic life receive no shock, and appear unaffected, except by the general febrile excitement. Patients need not be apprehensive of any ill results. Nevertheless, in some exceptional cases of the grave form, it becomes necessary to make use of very active means. During the twenty years that my attention has been fixed upon this disease, I think I have not seen a single case end fatally. Yet I have learned that the boy brought to M. Lasèque as an epileptic, died at the Hôtel-Dieu under the care of Prof. Rostan. Six weeks after he left my

wards, he was attacked with renewed convulsions, with cough, dyspnœa and profuse perspiration. An autopsy showed softening of the upper part of the spinal marrow and the ordinary lesions of pulmonary consumption. It was plain that the fatal termination was due in large measure to tubercles.

What is the nature of this malady? That it is a disease of the nervous system no one can doubt; and a disease to be classed with epilepsy, hysteria, eclampsia and catalepsy. It is, moreover, rheumatic in its nature, in my opinion. Blood drawn from a vein has the buffy coat, as in articular rheumatism; and in some cases, as M. Delpech remarks, rheumatism alternates in its occurrence with the spasmodic actions.

I have found great benefit in this disease from general blood-letting, and from cups applied along the spinal column; and I hesitate not to recommend them as possessing undeniable power, when not prohibited by the state of the general system. I have no misgivings, when I speak of the efficacy of blood-letting; for you know that I do not make too free use of the lancet; when I advise you, therefore, to open a vein, I am well convinced that it is a good therapeutic means. My belief in the intermittence of these rheumatic spasms has induced me to try the sulphate of quinia; and I have seen benefit from it. Yet it is not so sure a remedy as blood-letting. During the paroxysms the inhalation of chloroform is sometimes beneficial; and I should tell you that good effects sometimes result from opium and belladonna in moderate doses. But these agents should not diminish the value of blood-letting and sulphate of quinia; to the first place in the treatment they are well entitled.

S. C. C.

IV. *Irritating Substitutive Medication.* Translated from Trousseau and Pidoux, vol. 1, p. 474, et seq. (Continued from November No., page 373.) By D. I. McKEW, M. D., Baltimore.

WHAT shall we say of the virus of variola, vaccinia, scarlatina, rubeola and syphilis, which has not been already said so often, even to satiety?

What we see in man also holds good for the animal creation, and is recognized even in plants, the organization of which is so much lower; and in their pathological disorders, plants testify powerfully to the influence of the *quality* of the cause over the form of the disease. The wounds made by insects on the leaves and stems of plants are seen to produce morbid growths, the uniform characters of which recall at once the agent. Thus a certain form of gall follows the puncture of a given insect, and with a constancy so fixed that the skillful naturalist judges always from the form, color and volume of the tuberosity, the insect which has deposited its larva therein. These are not idle questions. They will serve to establish the specificity of duration, the great point of "substitutive medication."

In the examples which we have cited it would be a flagrant absurdity to attribute the effects solely to *quantity*, and not to the *quality* of the cause. It is, finally, as great an impossibility to produce symptoms of hydrophobia by any quantity, how great soever, of variolous poison applied to the human body, as to make with escharotics an eschar with the characteristics of malignant pustule.

But if it be absurd to refuse to the *quality* of the cause an immense share in the form of the effects, it is almost quite as ridiculous to wish to exclude the *quantity* of the cause from all participation in the production of the effects. *Quantity*, that is to say, the plus or minus in the cause, has a great influence over the *intensity* of the effects, but it cannot cause any difference in their *intimate nature*. Hitherto we have gone from the cause to the effects. The cause being well known,

well appreciated, if not in its intimate nature, at least in the periods of its application to the body of the patient, it has been easy to follow it in the phenomena of re-action which it has excited in the organism, and the special form of these secondary phenomena has been of easy determination. If all causes were as easily seized, there would be no difficulty, and the speciality of all diseases could be readily proven; but for a large number of diseases the cause is unknown, only the effect is present, and we must ascend from the known effect to the unknown cause. But let us remark, that the speciality of a disease is as well proven by the invariability of its forms, independently of the causes which produced it, as if we had known at once the effect and the cause. The causes of most diseases which manifest themselves by functional troubles of the abdominal viscera are perfectly unknown to us; but these functional disorders and the lesions connected with them are so unvarying in form that we quickly arrive at an affirmation of their speciality. Between Asiatic cholera, dysentery, dothinerterite, there are differences so marked, and the symptoms by which they are accompanied are so positive, that the most inexperienced doctor readily distinguishes the one from the other, and the very possibility of this distinction implies the idea of speciality; for there is no distinction possible in the absence of specific characters, and the establishment of these characters establishes *eo ipso* the specificity. Now, in the case of the three diseases of which we have spoken, it is certainly not by the phenomenal quantity, that is to say, by the intensity of each of the symptoms that the character of the affection is judged, but by the quality, *i. e.*, by the special form of certain phenomena independently of their intensity. Whatever we may do, we will never make cholera Asiatica of dothinerterite, sporadic cholera of dysentery, whatever may be otherwise the gravity of these different diseases. Each one will preserve its distinct features, its specific characters. It is sound philosophy to conclude from constancy in the effect a constancy in the cause. And it is finally no better logic to presume an identical cause for cholera and yellow fever, than to attribute variola and scarlatina to the action of the

same virus. The sectarians of Brown and Broussais, after having long fought against the cogent arguments which were ruining their dichotomous doctrine, found themselves at last forced to acknowledge the existence of special diseases; and, as if in admitting this principle alone, their system was not destroyed, they wished still to attempt a reconciliation of the doctrine of specialities with their narrow theories. What matters it to Brown that variola be a special disease or not? He cares nothing for it. It is a sthenic or asthenic disease; this is the only question which occupies him: Whence the indication to stimulate or debilitate? For Broussais it is the same. What matter after all if cholera differs in its forms from typhoid fever? It is but an irritation of the digestive tube which awakens different sympathies. Irritation is the common and culminant phenomenon. It alone is really the cause; it governs all; from it arise all the therapeutic indications. Such is the objection of Broussais, which we have not weakened—which we have given in all its strength; but which appears to us none the less feeble.

Doubtless, and we confess it freely, almost all the *modifiers* applied to the human body, create locally a common re-action, which it has been agreed to call inflammation or irritation. The whole question is reduced to the determination, whether this common phenomenon has really the pathological importance which has been accorded to it. Without doubt malignant pustule and furuncle, variola and impetigo, syphilitic chancre and herpes preputialis, acute laryngitis and croup, typhoid fever and “embarras gastrique,” catarrhal ophthalmia and gonorrhœal ophthalmia, have inflammation as a common character, as dulcamara and datura stramonium, chelidonium and poppy, eglantine and laurus cerasus have some common characters, since they are grouped in the same natural families; but what physician, what naturalist, would be foolish enough to attach but a secondary importance to those specific characters which play here a part so important?

Let us hear Bretonneau: “The obstinacy of a physician,” says this excellent practitioner, “who persists in recognizing

in bronchial catarrh and membranous angina but two unimportant shades of the same affection, is it not equal to that of a naturalist who would maintain that the viper is but a variety of the adder, and who, adducing in proof of his opinion the similarity in the mode of their circulation and in their generic characters alone, would regard the scales or plates which cover the head, the absence or presence of poison fangs as matters of very slight importance? What shall we say, however, to the antagonist of distinctions, when to his prejudiced eyes a viper and a rattlesnake are but over-grown adders? What shall we do? Insist on the difference of the effects which the bites of these reptiles produce, and, while we await the recognition of the truth by every one, hasten, if a poisonous bite have been received, to remove the cause of the dreadful disease by cutting out the small portion of living tissue which the poison has penetrated."—(Bretonneau, *Notes inédites sur les Phlegmasies spéciales.*)

We have purposely insisted on the question of the specificity of diseases, because this question governs pathology, and that we could not cause "substitutive medication" to be well understood, if we did not first establish this great pathological principle that *special modification corresponds to a special modifier.*

Finally, all irritating modifiers determine an irritation, the gravity of which, as local and general lesion, are subordinate to their nature, abstraction being made of the predisposition of the patient. The attentive pathologist can then, to a certain point, calculate the force of the irritating agent, and, though he cannot seize the cause, he may, nevertheless, appreciate the probable duration and gravity of an inflammation by experience and medical statistics. He sees that certain inflammations have a necessary march, and are to such extent fatal, that they have their origin, growth and termination in a determined space of time; that they have, as it were, a life like animals and plants; that others, uncertain in their duration, have sometimes an ephemeral existence, and sometimes are obstinately prolonged to the extinction of life, while again they appear and disappear without any regularity in their march. It results hence that, to the educated physician, the

probable duration and natural course of diseases are pretty well known. This knowledge, the most important for the therapist, that without which he cannot enter philosophically upon the least experimentation, is nevertheless that which is most neglected in clinical study. It is this which has been wanting, and which is still wanting, in all homœopaths.

The march and duration of a disease being known, if it were possible to place in contact with the inflamed tissue a modifier, of itself an irritant, and which would abridge the duration of the disease, would not a great therapeutic service be rendered? Now are things thus? Certainly. An inflammation seizes the conjunctiva; a collyrium of sublimate, nitrate of silver, calomel, red precipitate, even while it irritates for an instant the already inflamed part, nevertheless cures the existing inflammation.

This fact established, we must now study its laws.

[To be continued.]

CHRONICLE OF MEDICAL SCIENCE.

OBSTETRICS, &c.

I. *On the Treatment of Puerperal Eclampsia.* By Dr. PIRRIE.

[THE treatment of Puerperal Eclampsia is so interesting a subject to physicians everywhere, that we gladly republish the remarks of Dr. PIRRIE from the "Dublin Quarterly Journal." The principles of treatment advocated by him are not new to us; but as corroborative evidence of a practice rapidly gaining favor, we think they will prove interesting and acceptable to our readers.—EDS.]

Dr. Pirrie, Physician to the Belfast Lying-in Hospital, read before the Belfast Medical Society, the following *Observations on the Treatment of Puerperal Eclampsia*:

It is now about two years since I directed the attention of the members of this society, and my professional brethren, to the modification of the usual routine treatment of puerperal convulsions based upon more distinct and clearer views respecting the true pathology and etiology of this frightful and heretofore so fatal a complication of pregnancy and labor. The following extract is from the introduction to that paper :

“I have long been dissatisfied with the *routine practice*, and its results, in cases of puerperal convulsions. By routine practice, I need scarcely say that I refer to bleeding, which in its various forms has been recommended most urgently by almost all of even our modern authorities in obstetrics. Thus, we everywhere meet with expressions such as the following: ‘Bleeding is our great reliance.’ ‘The lancet is our sheet anchor; and blood may be taken to a very large extent.’ (Ramsbotham). ‘If there be a case in which the bold and daring use of the lancet is demanded, it is the case of puerperal convulsions.’ (Meigs). ‘The first thing to be done is to take away blood from the arm or temporal artery largely and in a full stream.’ (Churchill). ‘In the first rank, both as prophylactic and curative, are to be placed sanguine emissions.’ (Cazeaux). But that such was the universal practice, I need cite no authority, as I believe all of us have been in the habit of thus treating our cases of puerperal convulsions, as I was myself till some time ago, when I happened to have a case of convulsions occurring during the progress of Bright’s disease of the kidney in a male, and a case of puerperal convulsions in a pregnant female before labor, under my care about the same time. I was then most forcibly struck with the similarity of the signs and symptoms in the two cases; in both there was œdema of the face and upper extremities, accompanied by albuminuria, and the paroxysms of convulsions in both were absolutely identical. I treated them both on the same principles, of which bleeding formed no part, as being virtually different stages of the same disease, and had the satisfaction of seeing the case of puerperal convulsions recovering without a trace of remaining disease; and the case of Bright’s disease, although looked upon at one time as actually moribund, so far recovered as to be able to leave hospital in decidedly an improved state.”

Since that time it has fallen to my lot to see no less than *six* cases of *true* puerperal convulsions (including my hospital cases, and others which I saw in consultation with some professional friends), and as these would represent the average of cases occurring in more than 6000 cases of labor (Ramsbotham;) and as in all these cases I have carefully satisfied myself of

the true nature of the affection, and have, as far as possible in them all, endeavored to carry out the line of treatment I then indicated; and as I have every reason to be more than satisfied with the result, I think I am justified in again calling your attention to this subject, especially when I recollect with what anxiety I watched the result of this practice in my earlier cases, opposed as it was to the teaching of almost all, if not all, of our British authorities in obstetrics. Dr. Ramsbotham, in a paper published the other day, on a case of convulsions, reiterated his opinion that puerperal eclampsia is merely a modification of cerebral apoplexy, and to be treated accordingly.

That there is more or less of *cerebral congestion* accompanying the paroxysms of puerperal eclampsia, no one who has seen anything of the disease can deny; and I admit the possibility of this congestion being followed in some *rare cases* by effusion, or even rupture of some of the cerebral vessels; but I assert, nevertheless, that this cerebral congestion is *not the primary* disease, but is entirely secondary, and a direct consequence of the spasmodic action of the muscles of respiration interrupting the oxygenation and due circulation of the blood.

For the last twenty years it has been admitted, from the researches of Simpson, Lever, Cormack, Cazeaux, Blot and others, that *albuminuria* was often associated with puerperal convulsions; but I believe we are now justified in going a step further, and maintaining that every case of *true puerperal* eclampsia is not only accompanied by *albuminuria*, but is caused by it.

Now, if we bear in mind that in *albuminuria* we have the kidneys permitting the exudation of an important constituent of the blood, (serum), while at the same time they retain within it excrementitious matters which should have been eliminated, and that the blood, thus depreciated and contaminated, circulates with difficulty through the systemic capillaries, giving rise to œdema, and affords to the brain and nervous tissues a supply insufficient and inadequate for their nutrition, and the healthy performance of their functions,—we must admit, that a condition very analogous, if not identical with *anemia*, is produced. This explanation of the mode of production of puerperal convulsions is remarkably corroborated by the most interesting researches of MM. Kussmaul and Tenner on “Epileptiform Convulsions from Hæmorrhage,” published in the last volume of the New Sydenham Society. They have demonstrated, from many experiments, that insensibility, unconsciousness, and general clonic and tonic spasms (constituting the essential phenomena of puerperal eclampsia,) may be produced *at will*, by suddenly interfering with the nutrition of the brain,

and *could not* be produced by any amount of congestion or plethora.

Cause of Albuminuria.—Simpson, who was one of the first to point out the very close connexion that exists between albuminuria and puerperal eclampsia, although he hesitated to place them in the relation of cause and effect, described them as, perhaps, simultaneous or successive effects of one common central cause, viz., a pathological state of the blood, to the occurrence of which pregnancy in some way peculiarly predisposes.

I am more inclined to refer it to congestion of the kidneys, caused by a direct pressure of the *gravid uterus*, as suggested by Dr. Cormack. Many arguments may be adduced in support of this view of the mode of production of *albuminuria*.

1st. The vast preponderance in the numbers of *primiparæ* so affected, amounting to nine-tenths; in these the abdominal walls do not yield so rapidly, and, consequently, there would be greater pressure exercised on the renal vessels. This will also explain how excessive distention of the uterus, from *plural* pregnancy or dropsy of the amnion, is so often assigned as a cause of puerperal convulsions.

2nd. Another argument is, the rapidity with which the albumen disappears from the urine after delivery—"Sublatâ causâ, tollitur effectus."

Sir Charles Locock, in the article "Convulsions," in the *Cyclopædia of Practical Medicine*, in assigning mental emotions, especially depressing ones, as possible excitants of the paroxysm, says: "It has long been remarked, that unmarried women are more particularly liable to be sufferers from convulsions, from the shame and distress under which their children are usually born." Now, if we bear in mind, the efforts that these women are continually making to conceal their position from their friends and the public, we might, perhaps, conclude, that direct physical pressure had more influence in the production of the convulsions in these cases than mental emotions.

It will be also plainly manifest, how a stomach distended from over-repletion, and how constipated bowels, often assigned as causes of puerperal eclampsia, may really become so, by increasing the pressure upon the renal vessels. We may even adduce the *results of treatment* as an argument in support of this view of the etiology of the disease, viz., that no matter which treatment be adopted, once convulsions have occurred, they will in almost every case be repeated till the evacuation of the uterus effectually removes the pressure, and till the

resulting effects of that pressure begin to disappear from the blood and urine.

The treatment I adopt, and would strongly recommend, is, if not based upon, at all events consistent with these views of the pathology and etiology of the disease. It is briefly, first, *relieve* the pressure, not on the brain, but on the renal vessels; second, eliminate noxious matters from the blood; third, stimulate the kidneys to healthy secretion.

These rules are applicable under all circumstances, whether the convulsions appear during pregnancy, or labor, or after child-birth. Suppose puerperal eclampsia to occur, say in the eighth month of pregnancy, without any appearance of labor, we should first attempt to *relieve pressure* by acting freely on the stomach and bowels, thus reducing the contents of the abdomen, and at the same time fulfilling the second indication of eliminating noxious matters from the circulation; and we shall best restore healthy urinary secretion by the administration of the vegetable acids, say benzoic and tartaric acid, as recommended by Braun and Frerichs. Should these means fail, and the convulsions still continue menacing the patient's life, I have no hesitation in stating that we are more than justified in proceeding more effectually to fulfill the first indication—that of relieving pressure by bringing on labor, and carefully evacuating the uterus.

Should convulsions set in during the progress of labor, our object should be to hasten the termination of labor, and, by the use of the means suitable to each particular case, promote the prompt and careful evacuation of the uterus, and thus effectually meet the first indication, while, at the same time, by the administration of purgatives and vegetable acids, we attend to the other indications.

Chloroform has been highly spoken of by many as moderating the severity and frequency of the paroxysms. I have used it, and believe it to be a useful adjuvant when it is necessary to temporize in cases where delivery cannot be immediately accomplished; and may be safely used, as long as the patient retains any consciousness in the intervals of the paroxysms. In connexion with the administration of chloroform, I would direct attention to a most important fact, discovered by Kussmaul and Tenner, in the course of their experiments, viz., that convulsions could not be induced in animals that had been subjected to *etherization*.

I need not say, that I would entirely prohibit venesection, and would earnestly dissuade my brethren from resorting to general bleeding in *albuminuria*, either as a prophylactic or curative, as I firmly believe its tendency is to increase the severity

of the paroxysms, to debilitate the patient, and to delay the recovery, by the promotion of secondary affections in cases where it does not directly tend to produce a fatal result.

Local bleeding, leeches to the temples, or cupping the nape of the neck, with cold lotions to the head, may possibly be beneficial in moderating the secondary congestion where it is excessive. I have tried them, but I cannot say with any direct benefit.

In lieu of the warm bath, which must in all cases be troublesome and difficult to administer, and in many impossible, I would recommend sponging the surface with warm vinegar and water.

With respect to the treatment adopted in the six cases which have come under my observation since I last addressed this Society, without entering into details, I will simply state the results. In *three* cases, venesection to a greater or less extent was used; *one* of these, occurring in a neighboring village, was bled very largely before I saw her; the convulsions continued, and she died the next day. *Another*, who had but trifling œdema, and one slight convulsion before delivery, was also bled to the extent of sixteen ounces before I saw her; there was no return of the convulsions, but her recovery was slow.

In the third case, I, myself, while waiting for the forceps, took six ounces from the arm, with the view of moderating the secondary congestion, in a strong young woman from the country, in whom the paroxysms had been allowed to continue for several hours without interference, under the idea that she was suffering from simple epilepsy. She recovered well.

In the other three cases, the treatment I have endeavored to inculcate in this paper was closely followed out, and with the best results, although two of them were of the most formidable character; one of them occurring under circumstances that could scarcely be aggravated.

Of the six cases, four were delivered by forceps, one child dead; one delivered naturally, child living; and one by version, child dead.

I will conclude by recapitulating the more important points in the form of distinct propositions:

1. Convulsions are *not* caused by plethora or congestion of the brain, but by anemia or defective nutrition.
2. Depletion aggravates the tendency to convulsions.
3. All cases of true puerperal eclampsia are associated with and caused by albuminuria—
4. Which is most probably caused by renal congestion, the effect of direct pressure.

The indications for the rational treatment are :

1. To relieve pressure, the cause of renal congestion.
2. To eliminate noxious matters from the circulation, and
3. To stimulate the kidneys to healthy secretion.

P. S.—Since writing the above, I have had an opportunity (through the kindness of my friend Dr. Corry), of seeing a case of albuminuria with œdema occurring as early as the second month of pregnancy, when, of course, the pressure exercised on the several vessels would not account for the albuminuria. I do not consider that this very interesting case in any way invalidates the statements I have made above. I look upon this case as an accidental complication of pregnancy with previously existing *organic* disease of the kidney. It is the woman's *fourth* pregnancy; the others were unaccompanied by convulsion or œdema. Should convulsions occur, as most probably they will, early, the case would present a combination of phenomena almost identical with those observed in *true puerperal eclampsia*, whilst at the same time there existed marked points of differences in the history of the cases, and in the causation and sequence of the symptoms, sufficient to modify materially the prognosis, and, in a measure, the treatment also.

2. *Of the Misuse of the Forceps in Impracticable Labor.* By M. PAJOT, Prof. pro tem. (Championniere's Journal.)

A woman, in a dying state, was conveyed to the Hospital of the School of Medicine; she had already been twice delivered with instruments of a still-born child. The pelvis was surmised to be contracted, and a third accouchement, attended with greater difficulty, fully confirmed the supposition. The practitioner, who was summoned, fruitlessly applied the forceps, and persevered too long in the use of the instrument. Convinced at last of the inutility of his efforts, he called in a second surgeon, who repeated the application, with no greater success. As the patient was becoming exhausted, the assistance of two other surgeons was required, and they adopted the same plan, with no better issue. The unfortunate sufferer therefore underwent, without result, attempts reiterated during an entire night. When she reached the hospital, the pulse was not perceptible, the extremities were cold and insensible; the liquor amnii had escaped, the womb was powerfully contracted, and the bladder, in a state of semi-distention, formed above the pubes a soft, fluctuating tumor. On auscultation, M. Pajot failed in detecting any sounds resembling those of the foetal heart. A catheter was inserted into the urethra,

but it was impossible to introduce it into the bladder. M. Pajot discussed as follows the course that should have been adopted:

“Only one or two applications of the forceps,” said M. Pajot, “should have been attempted, and not four. The perseverance in the use of the instrument had been most pernicious, and its consequences to the patient were disastrous. In such cases, the surgeon should know when to discontinue active interference, and this is the great secret in difficult parturition. When the head presents, the forceps is applied, and should be at once withdrawn if the first efforts at extraction are unsuccessful. The surgeon should then ascertain, by auscultation, if the foetus is still living; if so, a second application of the instrument is allowable, with a view to preserve, if possible, both mother and child. After two such attempts, however, the latter is generally dead, and a third introduction of the instrument is therefore useless. The life of the parent and child cannot now both be saved: a painful decision must be come to, the perforation of the head of the foetus. For my part, added M. Pajot, I should not hesitate in adopting this course, were even the child living, and *a fortiori* when it is probably dead.” In the case under consideration, the Professor proceeded as follows:

“Guided by two fingers of the left hand, placed in contact with the head of the foetus, which had descended rather low, the operator inserted Blot’s perforator, and by a fortunate coincidence the instrument at once penetrated into the interval between two bones: in a few seconds the aperture was enlarged, and the cerebral substance disaggregated. The instrument was then withdrawn, and the cephalotribe was easily inserted in its stead. The crushing was at once proceeded with, blood mixed with cerebral matter escaped, and the head collapsed. At this stage of the operation, said M. Pajot, the instrument should be entirely locked at once; the purchase is thus as firm as possible, and there is no fear of the head gliding away after the destruction of the brain. The blades being satisfactorily placed, and the instrument locked, M. Pajot imparted to it a slightly rotatory motion. In the immense majority of cases the sacro-pubic diameter of the pelvis is that which is most diminished; if therefore the crushing is performed laterally, the head will be lengthened in the direction most unfavorable for extraction. Slight rotation of the instrument converts the antero-posterior into a diagonal diameter, and allows of the difficulty being overcome. In this case, slight traction, combined with the lateral motion described, sufficed for extraction of the head. This, however, was not all.

The foetus was large, and weighed over seven pounds without the brain. The shoulders were not extricated without trouble. M. Pajot passed, as he recommends in such cases, a napkin around the neck, and using it as a means of traction, disengaged first one shoulder, then the other, and thus concluded the operation. Tonics and stimulants were exhibited to the patient, but she fell into a state of collapse, and expired twenty-four hours after the first application of the forceps.

On dissection, a laceration of the vagina was discovered, which, from the appearance of the coagulum, must have taken place twenty hours at least before death.

This sad but most instructive case affords additional proof, if any were required, that instrumental labor continued for three or four hours is liable to occasion laceration of the vagina. This injury and even laceration of the womb have been known, it is true, not to occasion death, but it is generally fatal. In sixteen instances of the kind, M. Pajot numbered sixteen deaths. Therefore, said the Professor, when labor is impeded by absolute or relative narrowness of the pelvis, the surgeon who is obliged to apply the forceps should never forget that, after moderate and very gentle efforts he must lay aside the instrument: let the patient be placed in a bath, and in an hour a fresh application of the forceps may be made. Should this fail, the accoucheur must resort to other means, a rule not perhaps absolute in practice, but one less likely to cause injury than the opposite precept. If the foetus has ceased to live, the head should unhesitatingly be perforated and crushed. In some few instances, M. Pajot has made a third attempt with the forceps, but never more, and he is convinced that, by following this line of conduct, he has preserved his patients from the perils of laceration of the vagina and from puerperal symptoms.

3. *On the complete Occlusion of the Os Uteri during Pregnancy, and on the Operation required under the circumstance.*
By M. DEPAUL. (Imperial Academy of Medicine.)

The existence of this morbid condition, said the learned obstetrician, has been disputed by most authors, but accident having brought three unquestionable cases under his personal observation, he considered it might be interesting to lay them before the Academy.

The first patient was a woman whose pelvis, much contracted, had required in a first accouchement the use of the perforator. In the present instance, labor had lasted forty-eight hours,

when M. Depaul was summoned to the case by Dr. Remondet. In addition to the deformity of the pelvis, M. Depaul ascertained with the finger and with the speculum, that the os uteri was utterly destitute of any orifice or depression indicative of its perviousness. Two days were suffered to elapse, when, having consulted Professor Dubois on the subject, M. Depaul determined upon performing an incision in the centre of the part of the womb which protruded into the vagina. The operation was fully successful, and demonstrated the correctness of the diagnosis, but the narrowness of the pelvis again necessitated the destruction of the child.

The second case related to a woman who had borne several children, and had reached the seventh month of pregnancy. She was admitted in September, 1855, into M. Trousseau's wards in the Hôtel-Dieu, and had for a long time suffered from obstinate and almost uninterrupted vomiting. All the remedies prescribed having failed in arresting the symptoms, M. Trousseau requested the opinion of his colleagues at the hospital, to discuss the propriety of inducing premature labor. M. Depaul who was present at the consultation, having proposed further delay, the operation was adjourned. The vomiting, however, continued, and on the 11th of October, the patient having been seized with convulsions, M. Depaul was summoned for the purpose of inducing premature delivery. On examination *per vaginam* and with the speculum, he found that the internal orifice of the cervix was entirely closed by a thick and solid transversal septum. Several of the persons present having likewise ascertained this condition, he divided the unnatural partition, and when sufficient dilatation had taken place, he concluded the accouchement as promptly as possible with the forceps. The eclamptic attacks, however, persisted, and the patient died two days after.

The third case was a primiparous woman, who had never labored under uterine disease and in whom no cauterization of the cervix had ever been performed. The last period of pregnancy had arrived when M. Depaul discovered, during labor, a complete obliteration of the os uteri. After several hours of ineffective pains, labor ceased to progress, and the case presenting no urgency, M. Depaul considered himself justified in refraining from active interference. Six days later, labor again set in, and as five hours elapsed without any sign whatever of dilatation, and as the patient was becoming exhausted, M. Depaul performed with a bistoury incision of the cervix. Parturition was subsequently accomplished without difficulty, and its consequences were of the simplest.

From a comparison of these cases, which have fallen under

his own observation, with others to be found in the records of science, M. Depaul drew a sketch of the history of complete occlusion of the os uteri in parturient women. The obstruction is usually the result of some local injury, such as obstetrical manipulations in previous confinements, surgical operations, cauterizations, etc., and may occupy the external or the internal aperture of the cervix. In either case it can be removed but by an operation, for the performance of which the surgeon must take into account the time which has elapsed since the beginning of labor, the strength or weakness of uterine contraction, the degree of participation of the entire system in the local function, and the condition of foetal circulation. He must not forget that convulsion or rupture of the uterus may be the consequence of delay too long protracted.

The artificial aperture must be made on the spot of the occlusion.

The patient having been placed in a proper attitude, a common hysterotome, wrapped round with a few turns of surgeon's lint, up to within four lines of its point, is guided along the left fore-finger, placed in contact with the cervix. The incision should not exceed three lines in extent, should run from side to side, and divide in succession the different layers of the structures. From time to time, the depth of the incision should be ascertained with the finger, and the thickness of the remaining partition estimated. An incision of about two or three lines should further be performed at each extremity and in the middle of the posterior lip of the artificial aperture. The cavity of the womb being laid open, the orifice must then be enlarged with the finger, and nature entrusted with the continuance of labor, the forceps being however resorted to in case it should be too long protracted.

The operation causes scarcely any pain, an insignificant quantity of blood escapes, and its consequences are destitute of importance. Experience shows that the occlusion has no tendency to return.

4. *Puerperal Convulsions successfully treated by Subcutaneous Injections of Morphia.* By Prof. SCANZONI, of Wurtzburg.

Since the attention of the medical profession was first directed by Dr. Wood, of Edinburgh, and more lately by Hunter and Béhier, to the advantageous effects of subcutaneous injection, especially of narcotics, Professor Scanzoni has employed this method with success in numerous cases of neuralgia, hyperæsthesia, etc.; but he attaches especial importance to the follow-

ing case of puerperal convulsions, because it seems to prove, in accordance with the views laid down by Hunter, that the subcutaneous application of narcotic agents furnishes a means of acting on abnormal irritations of the brain with greater rapidity and certainty than the administration of the same remedies by the mouth. It will, doubtless, be admitted that opium, and its different preparations, deserve the first place in the treatment of puerperal eclampsia. In his own experience, the observation of a large number of cases has convinced Professor Scanzoni that a kind of intoxication produced by opium leads with more certainty to a favorable termination than any other means recommended in this terrible disease. But, unfortunately, it is not always possible to administer a sufficient quantity of opium or morphia; sometimes the comatose condition of the patient, at other times the rapid succession of paroxysms, prevents administration by the mouth; and opiate enemata are occasionally rejected as soon as they are received. The subcutaneous injection, however, supplies the means by which these difficulties may be overcome, and a sufficient quantity of opium introduced into the system to render its effects certain. Numerous experiments have convinced the author that, although the effect of this method is not always persistent, (the neuralgiæ, for example, are not always cured by it,) yet there are constantly produced—within a very short time, often a few minutes after the injection—certain phenomena, which can leave no doubt as to the action of the opium upon the brain. Such symptoms are drowsiness, giddiness, headache, sickness, feeling of constriction in the throat, even vomiting, and depression; or, if the dose is large, somnolence. These facts, taken along with the known effects of the subcutaneous application in delirium tremens, mania, chorea, tetanus, etc., induced him to try the same treatment in puerperal convulsions, and with the most satisfactory results. After three injections of meconate of morphia there occurred only two attacks in nine hours, while previously there had been three attacks in an hour and three-quarters. This diminution of the convulsions after the injections is so much the more remarkable, since experience has shown that, as a general rule, the paroxysms become not only more violent, but follow at shorter intervals as the labor advances. And although the author does not imagine that he has discovered in the subcutaneous injection an infallible panacea for this dreadful malady, he is of opinion that the following case should induce physicians to give this means a trial:

Case.—D——, aged 21, primipara, strong and robust, was brought into the lying-in ward at a quarter to eight o'clock on the morning of June 8th, 1859. Labor had commenced in the

night, and she had been seized with nervous paroxysms and loss of consciousness; no account was given of the nature of the attacks; the patient remembered nothing of what had occurred during the night. The whole body, and especially the lower extremities, were œdematous; on the right side the tongue showed marks of being bitten by the teeth; the uterus corresponded to the pit of the stomach, and seemed sufficiently consistent; sounds of the foetal heart distinct. On examination, the os uteri was dilated to the size of a sixpence, the bag of waters were partly formed, and the head presented; the urine was very albuminous, and exhibited under the microscope numerous fibrinous cylinders. At eight o'clock she was seized with a second convulsive attack, which was of a very marked character, and lasted for some minutes. On recovering consciousness, she could answer questions, although slowly. A third attack succeeded at a quarter to nine, a fourth at a quarter to ten, a fifth at a quarter to twelve, and a sixth at five o'clock—the last the most violent. After the fourth paroxysm, consciousness did not return, and the breathing became stertorous. At ten o'clock, she was bled to about eight ounces, an enema with twenty-five drops of laudanum was given, the body was put into a warm bath, while cold irrigation was applied to the head. As opium could not be administered internally, a solution of the meconate of morphia was now, at three different times, injected under the skin, the quantity amounting in all to about ten grains (seventy-five centigrammes) of opium. The labor advanced very slowly. At three o'clock next morning, the membranes burst; the os dilated to the size of a half-crown; the head still high up above the brim; sounds of the heart very distinct. After this period, the dilatation went on more quickly; at seven o'clock, the os was larger than a crown piece, very extensible and dilatable, the head high up and immovable; complete loss of consciousness, profound coma. In these circumstances, which left little hope of saving the patient, and in spite of the high position of the head and the incomplete dilatation of the os uteri, it was decided to employ the forceps. Their application was by no means easy, but the extraction presented no difficulty. After a few tractions, a foetus was born, which breathed feebly at first, but soon began to moan vigorously; the placenta followed. During the operation there was no paroxysm. Some wine and ten drops of tincture of amber and musc were now given to the patient, which revived her a little, but did not restore consciousness. At eleven o'clock, a seventh attack came on, but was slight and short; after which she became excited, and tried to escape, but towards morning she grew calm. At nine in the morning, she

could answer questions put with a loud voice. During the whole day she remained like a drunken person; pulse 128. The music was stopped; nothing but lemonade given. Towards evening the abdomen was somewhat painful. During the night there were several slight attacks of mania; she constantly attempted to escape. In the morning, she answered rationally; pulse 108. The œdema had diminished; the abdomen was still tender; there was difficulty of breathing; and numerous râles, fine and coarse, in the lungs. Warm bath, lemonade and expectorants were prescribed. In the evening, the patient was completely herself again; pulse 132. June 11th and 12th: She slept well during the night, the expectoration becoming easy, and the pain of the abdomen relieved by fomentations and poultices; pulse 120; the urine contained little albumen, and no fibrinous cylinders. June 13th: Good condition; œdema gone; abdomen soft; some incontinence of urine during the night was relieved by leaving in a catheter. All medicines were now suspended; the patient was put on good diet; and ordered to take every morning a glass of chalybeate mineral water. On the 17th, there was no albumen found in the urine; and on the 21st, the patient left the hospital with her child, being advised to continue the use of steel for a considerable time.—*Bull. Gén de Thérap.*, March, 1860.

5. *Performance of the Cæsarian Operation after Death.*

Several journals recently related that, in one of the Western Departments of France, a sister of charity, at the instigation of a clergyman, had performed the Cæsarian operation with a carving knife upon a gravid woman who had died some hours previously. We cannot vouch for the truth of the story, but the publicity it has received has induced Mr. Laforgue, Professor of Midwifery at the Toulouse School of Medicine, to make on the subject some very judicious remarks, which we reproduce from the columns of the *Abeille Médicale*:

“While the medical profession,” said Mr. Laforgue, “is as yet in doubt with regard to the performance of the Cæsarian operation after the death of the mother, theologians entertain very positive ideas on the subject, and have preserved in its full stringency the ancient law which prohibits the inhumation of a pregnant woman before she has been delivered of her child, *antequam partus ei excidatur*. (Digest, lib. ii., cap. 8.)

“The legislature having permitted this law to fall into disuse, physicians refrain from an operation which they generally consider useless; theologians, on the contrary, are ever ready

to act, being inspired with a hope of promoting the salvation of the child by administering baptism. The conduct of both is to be regretted; it is, on the other side, founded on a true, but exaggerated principle, and I consider it may be useful to indicate briefly what should be the practical solution of this much disputed question.

“The intervention of persons unacquainted with medicine being extremely dangerous, the operation should never be performed but by medical men. They alone are possessed of the special knowledge requisite for the appreciation of the signs of death, of the proper time and procedure for the operation. This being an unquestionable fact, it follows as a necessary consequence that all practitioners should consider it a professional duty to perform the Cæsarian section after the death of women who have reached the last months of pregnancy.

“The numerous instances of posthumous births resulting from this operation leave the surgeon no option in the matter; both humanity and religion command as an imperative duty to attempt to save the child who outlives its mother. On none but on the medical practitioner devolve the duty and honor of making an effort to preserve a being endowed with the principle of life. Should he fail, his attempt will be deserving of praise; and, if successful, he will have accomplished one of the noblest acts of his calling.

“Seven years since, (May 26th, 1853,) I successfully performed the Cæsarian section on a woman who died at an advanced period of pregnancy, and a quarter of an hour after her death I extracted a living child, who bears the name of Cæsar Bénoret, and is now a fine, vigorous boy, in the enjoyment of perfect health.

“This case, which was recorded in most of the periodicals of the time, (*vide* *Union Médicale*, An. 1853, vol. vii,) may, without impropriety, be mentioned here, in order to inspire professional men with a confidence, which will be doubtless often disappointed, as I have personally experienced, but which will certainly be deemed calculated to justify the expediency of an operation which I consider a duty imposed on the practitioner.”

We have always shared the opinions expressed above, and we had an opportunity of applying them practically, some 20 years since, in a case which presented some interest with regard to a question of inheritance.

A consumptive woman, residing in a country-place in Brittany, summoned us one day, in order to claim at our hands one last service. She had been married twelve months before, was six or seven months pregnant, and the thought that her

child, which she felt to be living, would follow her unbaptized in the grave, was more cruel for this Christian soul than even the approach of death.

We promised her that her pious wish should be accomplished. The husband had further insisted upon the timely performance of the Cæsarian section. It was instituted according to the rules of art, and a living child, which received baptism, was extracted. This, however, was a part only of the delicate duty we had undertaken. We had further to declare whether this living child was *likely to live*. If so, he would inherit his mother's property, and transmit, in the probable case of not out-living the father, one-half of the inheritance to the latter and his heirs. If, on the contrary, the infant was not *viable*, the property of the mother was to revert to her relatives. Such was the issue of the case—the infant was puny and extremely feeble. We declared it not likely to live, and it expired after a few hours, never having been born to its civil rights.¹—*Championniere's Journ.*

6. *Obstetrical Excerpts.*

On the Effects of Chronic Lead Poisoning on the Fœtus.
By M. PAUL.—M. Paul's attention was first called to this subject by the case of a female type-polisher, who, prior to her exposure to habitual contact with lead, had borne three fine children, while of the ten pregnancies which had since then occurred, eight were attended with miscarriage, the only child born at full time dying when five months old. She informed him that almost all the women similarly employed either miscarried, or were unable to rear their children. Investigating the subject farther, he found that these statements were founded on fact, and that not only was this hereditary influence imparted by the mother who had been working with lead, but also by the father, when the mother herself had not been exposed to such influence. Of this latter fact he has not been able to obtain more than 7 instances; but altogether his paper is founded upon 81 cases which he has investigated. In some instances the effect upon the fœtus has been produced after but slight exposure, and in which the death of the infant has been almost the sole accident resulting from such exposure. Of the 81 individuals, male and female, interrogated, in 29 pregnancies occurred while engaged in manipulating the lead,

¹ According to the English law, it is not necessary that a child, when born, should be capable of living, or *viable*, in order that it should take its civil rights. But, in France, by Art. 725 Code Napoleon, no child born alive can inherit, unless it be born, as the laws term it, *viable*.

these pregnancies amounting in all to 123. The results were 64 abortions, 4 premature labors, and 5 born dead, that is 73 children dead prior to delivery! Moreover, 20 children died in the course of the first year, 8 in that of the second, 7 in that of the third, and 1 later. There are 14 living children, but 10 only more than three years of age. In 15 instances, too, the women suffered from metrorrhagia, which was probably, in fact, abortion—for the saturnine poisoning, in the degree in which it causes the death of the product of conception, does not usually disturb the menstruation itself, either in girls, in persons who have not borne children, or in the intervals of pregnancies. The author is still engaged in investigating the entire subject, but in the meantime desires to call attention to what he believes to be a new subject of inquiry.—*Archives Générales*, tome xv. pp. 513–536.

Deodorant Preparations for the Lying-in Chamber.—It would be needless for me to give the details of the various means adopted and of the different substances used, so I shall content myself with stating the composition of a powder, which I have found in every way well suited to the purpose:—Calced oyster shells, lb. j.; oil of tar, gr. lxiv.—Mix. This powder may be variously applied; the following method will be found effectual, if not the best that can be adopted:—One table-spoonful of the powder is to be mixed with from two to four table-spoonfuls of the finest dry bran, and spread between the folds of the ordinary pudendal napkin, so that one ply only shall be left between the powder and the patient; the napkin may be stitched along either extremity if required on account of the escape of the powder; and it is to be pinned before and behind to the ordinary obstetric binder. The powder and napkin must be renewed as often as the nurse thinks proper.

The advantages of this powder over that of any other, and over the much-talked-of one of MM. Corne and Demeaux in particular, is—1. That it does not cake or set, and adhere to the napkin like powders prepared from the sulphate of lime. 2. That its absorbing property is not inferior to, nor is it more caustic than, those prepared from sulphate of lime. 3. That powders prepared from sulphate of lime and tar, or oil of tar, retain the peculiar penetrating odour of tar; whereas, the powder prepared from calced oyster-shells and oil of tar, has not any smell of tar, but a mild, agreeable, and fragrant smell of the finest oil of peppermint; and, although the oil of tar is so altered in smell by the calced shells, its deodorising property is rather increased than otherwise.

Liquid Deodorant Preparation.—For the deodorisation of liquid fæces, or where a fluid form is preferable, as, for instance, as a vaginal injection in cancer of the uterus, I use the following preparation, and one great advantage of it is, that it may be dispensed by chemists, the same as any other prescription: R Tinct. camphoræ, tinct. myrrhæ, aa, ʒiij.; lin. saponis, ʒij.; acidi acetici glacialis, mxx.; ol. picis, ʒj. Add the liquids in the above order and agitate. The product ought to be of a pale sherry-wine color, and perfectly clear. Each tea-spoonful contains about ten minims of oil of tar.

Treatment of the Freckles of Pregnancy.—When freckles, which are of such common occurrence in pregnancy, persist after delivery, M. Hardy recommends the following lotion:—Dist. water, 125 parts, corrosive sublimate $\frac{1}{2}$ part, sp. wine, 9 parts, to dissolve it, sulph. zinc, acetate of lead, aa 2 parts. When this application fails, Barèges or Luchon water, applied as local douches to the parts affected, induce slight inflammation, which facilitates the absorption of the pigmentary deposits.—*Bull. de Théráp.* April, p. 362.

Affection of the Hip-joint as a consequence of Uterine Disease.—Dr. Hoppe calls attention to the fact that in the course of uterine disease, there may be well-marked hip-joint disease set up, as indicated by pain and tenderness in the region of the joint or the trochanter, and impeded movement, owing to the contraction of the adductors and flexors. Both sides may be affected, either simultaneously or in succession. It may exist only in a trifling degree, and when it prevails to a greater extent it may go on afterwards independently of the uterine affections. In moderate cases the affection of the hip-joint disappears with that of the uterus.—*Berlin Med. Zeitung*, No. 2.

BIBLIOGRAPHICAL RECORD.

- I. *An Elementary Treatise on Human Anatomy.* By Jos. LEIDY, M. D., Professor of Anatomy in the University of Pennsylvania, &c., &c. Philadelphia: J. B. Lippincott & Co., 1861. Pp. 663.
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Those conversant with the several excellent systems of human anatomy which are in use in this country, might have questioned the necessity of another, and might have doubted the expediency of sub-dividing a field already partitioned out into so many portions. It is not our business to enter at length into the consideration of these points, or to indicate in detail what we regard as deficiencies or redundancies in the textbooks on the subject. Suffice it to say, that we believe there was room for another, and we have therefore to ascertain whether or not the work before us is what it ought to be to fill the void which we have assumed to exist.

Scientific works are written for three classes of individuals: for the scholar—using the word in its restricted sense—for the tyro, and for those who not having time or inclination to master technicalities, desire a general acquaintance with a subject. For the first class, the work must be profound and thorough; for the second, elementary and simple; for the third, popular and perhaps superficial.

The work before us was written for the second of these classes, (the student,) and should therefore, in accordance with our ideas, be elementary and simple. The medical pupil in this country runs his course in three years—perhaps less—and he has no time to acquire anything more than the mere rudiments of any of the medical sciences. The books, therefore, that are intended for his use, should be clearly and concisely written, but at the same time should contain those essential features of the science, a knowledge of which will prevent the future practitioner from the perpetration of serious blunders. In the

treatise now under notice, we find all these qualities. It is perspicuous, it is terse, and above all, it is an ample guide to the practical details of anatomical science, and the student who acquires what it contains, may confidently be assured that his knowledge is fully sufficient to carry him safely through all the difficulties likely to fall in his way.

It is a mark of the highest ability to be able to divest a science of its superfluities or refinements and to bring it down to the requirements of those who have to make a practical use of it. To do this, necessitates a thorough knowledge of the subject, together with a clear and comprehensive understanding of what is wanted; in short, profundity and common sense, that combination so rarely met with, and when found so happy a gift to its possessor. We find that the author is equal to his task, and that he has produced a work which not only shows how good an anatomist he is, but one which also evidences how well he knows the wants of the medical student.

Let us point out some of the characteristics of the treatise before us. In the first place, it is concise. The descriptions are clear and to the point, and non-essentials are passed over. This is especially the case with the bones, and those bugbears of the student, the temporal and spheroid bones, can be understood without difficulty in a short time by any one of ordinary ability. In the second place, considerable prominence is given (as was to have been expected) to histology, and all the tissues are described clearly, but with a sufficiency of detail in a work devoted mainly to descriptive and surgical anatomy. In the next place, the nomenclature is Anglicised. This is a feature about which there will be great difference of opinion, and one which, we must admit, we think of doubtful propriety. Occasionally we have rather awkward expressions, as *terete muscle*, *profound artery*, &c. We think that specific names make more impression on a student's mind, than such general ones as *straight muscle* and others of the kind. The objection, however, loses much of its force from the fact that all the synonyms are given at the bottom of the pages.

The principal words of a paragraph to which the descriptions relate are printed in larger type and are accentuated. By these means the attention of the learner is arrested and at the same

time he is made acquainted with the correct manner of pronouncing the words.

The publishers have spared no expense in the getting up of the volume. The type is beautifully clear, the illustrations are wonderful specimens of artistic excellence, and are scattered profusely throughout the work, and the paper is of a quality rarely met with in text-books. We are sure that all who see the work will agree with us in the opinion, that no medical work hitherto published in this country can compare with it in the respects we have mentioned, and will join with us in thanking the publishers for the comfort and even luxury with which it may be perused.

In conclusion, we have only to repeat what we have already said, that the work is one which cannot fail to be useful, and we are sure that it will be thankfully received by those for whom it is mainly intended. The learned author has rendered a service to the student scarcely less important than those he is every day conferring, by his higher and more abstruse studies, on the masters in science, and we trust that, besides adding to his reputation, it will yield him that substantial reward which the more elevated scientific researches so rarely, if ever, afford.

W. A. H.

II. *Statistical Report on the Sickness and Mortality in the Army of the United States.* Compiled from the Records of the Surveyor-General's Office, embracing a period of sixteen years, from January, 1839, to January, 1855. Prepared under the direction of Brevet Brigadier General THOS. LAWSON, Surgeon-General United States Army, by RICHARD H. COOLIDGE, M. D., Assistant Surgeon United States Army. Washington : A. O. P. Nicholson, Printer. 1856. Pp. 703, 4to.

A Second Volume of the Same, embracing a period of five years, from January, 1855, to January, 1860. Pp. 514, 4to, with Outline Maps.

These volumes were compiled under instructions from the Secretary of War, in compliance with a resolution of the Senate

of the United States, demanding a statistical report on the sickness and mortality in the army. We are indebted for the copies in our possession to the politeness of Surgeon Robert C. Wood, U. S. A. They were not received in time for the extended notice they so fully deserve; for the rapid glances we have cast over them have sufficed to show us that they are exceedingly valuable. The reports of the various medical officers are not confined to mere matters of sickness and mortality; but they go largely into such as pertain to the physical geography of all portions of our national domains. They contain a vast deal of useful information on the topography, soil, climate, flora, fauna, and every branch of the natural history of the regions descanted upon. We know not any work which would be more useful to the enterprising adventurer, who would wish to build up his fortunes by establishing himself in a wild western home, than this.

In mere professional matters, the work is very interesting to professional readers. We can bear ready testimony to the zeal and ability with which the medical officers perform their arduous duties: we are confident that no service in the world can boast of having a body superior to them. This is owing to the facts that the government has put them upon a footing sufficiently respectable to command good officers; and that it allows them to select the members of their body by merit only. In the former respect, the government, strange to say, has done much less for the medical corps of the navy, than that of the army. This inequality may be accidental; but it is more probably copied from British customs, which had neither justice nor wisdom to commend them. The medical officers of the army under British authority were always on a better footing than their brethren in the navy; the consequence was, that while many clever men entered the army, those who entered the navy were often held pretty much in the estimation of those bad boys who are sent to sea, *because they are fit for nothing else*. We do not know whether assistant surgeons in the British navy have yet escaped from the steerage, and the unfit association thus thrust upon them; but we do know that the American assistants, who have, are vastly their superiors, in manner, character, general and professional intelligence.

But while in the British navy the assistant surgeons went so low in the official scale, some of the superior medical officers attained to very high collateral rank, so that while the assistant was very much humbled in the beginning, he had something to hope for in the end; in our service there neither is, nor was, high position for any medical officer, while the junior was, in times past, as much humbled as his British brother.

In our army the Surgeon-General ranks very properly with a Colonel; (the present incumbent holding the brevet rank of Brigadier General for distinguished services in the field;) in the British army, his position is higher; in our navy, the Surgeon-in-Chief, at the head of his department, has only the rank of a Commander, or of a Major in the army. Now the collateral rank held by medical officers does not give them military authority; it only gives an amount of respectability under that form which is most necessary in military association; and by every principle of justice, the Surgeon-in-Chief, and every Surgeon-of-the-Fleet, should be on a footing with Captains, (Colonels in the army,) not rivalling them in authority, but standing side by side with them, in the estimation of their brother officers and of the public.

Both the army and navy want first-rate medical officers; but the navy in this respect more than the army, to wit, that the naval surgeon is, and ought to be, a fit representative of American general and scientific intelligence abroad. It is to the interests of the service, and to the credit of the government to elevate him; it advances no interests, and reflects no credit on any quarter to withhold from him that comparative respectability to which he is so well entitled.

In both army and navy the *esprit de corps* is eminently cherished, and the officers have a common pride in the reputation of the members of their respective bodies. The Examining Boards are very particular in the admission of candidates, regarding moral character, physical ability, and general education as of almost equal importance with professional intelligence. The rigor of the boards is often construed into harshness and injustice, which faults may sometimes occur; but if they do, we believe it is very rarely and unintentionally. Unequal action should be avoided as far as possible, and irrelevant ques-

tions dispensed with, except in regard to assigning relative rank. We have heard of candidates being examined pretty closely upon botany and history, while not one question was asked upon materia medica and therapeutics. We believe that sometimes views of the examiners are taken too much for absolute facts, while different views, on the part of candidates, are held to be errors. Now ignorance upon a subject is one thing, and difference of views another, and the latter should be treated liberally. A candidate who does not know, for example, that the tincture of iron is reported among very many physicians to be the best remedy for erysipelas is guilty of the sin of ignorance; but one who knows what is taught of it, and yet holds that it is inferior to some other remedy designated, is worthy of a respectful hearing.

It is not many years since, as we are credibly informed, a candidate was asked by an examining surgeon, what course he would pursue, if called to treat a penetrating wound of the chest, where the patient was prostrate, or sinking rapidly, from internal hemorrhage. The question gave rise to the following dialogue: Candidate: "I would give the patient something to sustain him for a time, and bleed so soon as there was sufficient re-action." Examiner: "Would you not tie up both arms, and bleed freely at once?" Candidate: "I would not, sir." Examiner: "Then you would lose your patient." Candidate: "I thought, sir, it was a rule in surgery, in every case of severe injury, to wait for some re-action before drawing blood." Examiner: "There are exceptions to all rules, sir, and this is one of them."

We ask our readers, who had the right of it? Why, says one, derivative bleeding is necessary in wounds penetrating into the lungs, for obvious reasons; and therefore the examiner was right. But, says another, the man was already sinking rapidly, and to take blood at that time, would be certainly to take away the *vital spark* with the vital fluid. The *pros* and *cons* both admit of argument. Each side has a show of reason; but when treating such case, one wants positive knowledge, not reason. Reason teaches us to concur with the candidate. It taught the examiner differently. We must have another teacher, which teacher can only be experience. From our own handling of

Vulnus Punctum and *Vulnus Sclopeticum*, we should say, bleeding would be inadmissible in such a case. It would take a great mass of testimony to change our conviction on this subject. That the practice demanded by the examining surgeon was followed by some surgeons during the Crimean and other wars, we admit; that it was the most proper or successful practice, is just what we do not admit.

Upon this special subject, we refer our readers to observations by Dr. Frazer, physician in the London Hospital, published in the *Proceedings of the Royal Medical and Chirurgical Society*, June 22d, 1858. Dr. Frazer was also a Crimean surgeon. We have not space for any of his interesting facts upon the subject of "penetrating wounds of the chest," evidently derived from close observation; but we subjoin the following paragraph, involving the *questio vexata*, as we find it in Ranking's *Abstract* for January, 1859:

"The author (Dr. Frazer) next adverted to what has been, and is still asserted by many to be, the 'sheet anchor' in the treatment of penetrating wounds of the chest, viz., venesection. He gives the opinions of others, and expresses his own doubts as to the prophylactic power of venesection in obviating the tendency to inflammatory action, or in arresting its progress, or in removing its effects when present. In reference to treatment, he recommends the removal of foreign substances, and all other causes of irritation, when practicable, from the wound. When the wound is small, and especially if there should be two openings, the closing of the anterior is to be attempted, and if there be no sign of effusion, both may be closed, and in all cases absolute rest, cooling beverages, and moderate nourishment are called for, avoiding over-stimulation. Bleeding, mercurialization, narcotism and antimony, the old elements of treatment, may, under the direction of sound skill and under special circumstances, become advisable; but *their routine application is second only in mischief to the injury itself.*"

We have italicized the last sentence, which conveys a most wholesome lesson, and which, with every sentence in the paragraph, meets our unqualified approbation. To say the very least, the views of the candidate have quite as much support from reason and experience as those of the examiner, and we certainly think they should not have been dogmatically rejected as erroneous.

Notwithstanding such passages as this, we believe that the boards are uniformly composed of honorable and fair men, who do their duty in a proper spirit. Now and then a candidate may pass who ought to be rejected, or one be rejected who ought to pass; but this is because there is always some equivocation in human affairs, and not because the examiners are unfair or partial, designedly.

In this hasty sketch we have scarcely adverted to the primary business of the physician, which is assuredly the treatment of disease, as practiced by the medical officers of the army. Any practitioner may derive useful information from the reports, as to the treatment of some of the most formidable or familiar diseases known in North America. Cholera, yellow fever, congestive, intermittent, remittent and typhoid fevers, together with dysentery and diarrhœa, are the diseases most discussed, and most generally treated, and usually with the most creditable success. Among the multitudinous causes of disease, one report, from Fort Columbus, New York Harbor, speaks of men being attacked with sporadic cholera, after eating clams or muscles found on the beach. The cause is certainly adequate. During the early autumnal months, though they have *R*'s in them, we have to treat annually violent cases of cholera morbus in this city, caused by eating oysters before the weather becomes thoroughly cold. Some of the cases put on the semblance of Asiatic cholera. We warn our acquaintances from year to year of the danger, but *only experience* will make them heed. They will take oysters and physic; but they will not take advice—perhaps for the sufficient reason that it is given for nothing.

In conclusion, we think it proper to remark that Dr. Coolidge, the compiler of these volumes, has done his share of the work in a manner eminently creditable both to his diligence and to his judgment. His brother officers, the profession at large, and the great public must acknowledge their obligations to him, as well as to the gentlemen who supplied him with so great a mass of valuable materials.

R. McS.

III.—*Introductory Lecture, delivered in the University of Maryland*, by Prof. EDWARD WARREN.

WE have received, since our last issue, an "Introductory Lecture," delivered in the University of Maryland, by Professor Edward Warren, whose appointment to the Chair of Therapeutics and Materia Medica in that Institution has been so recently a subject of congratulation.

After a very glowing tribute to the memory of the departed teachers in the School, Professors Roby and Frick and Dr. Berwick B. Smith, the orator undertakes a "Defence of Medicine, as a labor of love, and with the hope of inspiring those who but stand upon its boundaries with a proper appreciation of the beauty and grandeur of the vast domain they would explore." He demonstrates the *fallibility*, the *weakness* and the *uncertainty* of reason, which has sounded the argumentative tocsin of opposition; and shows that, without the positive sanction of *revelation* or *experience*, reason has no claim upon the confidence of men, and that the reliability of fundamental ideas or "principles" must depend upon some practical demonstration of their truth. Heterodox medical theories, or so-called systems, fail upon the application to them of the absolute test of experience; whereas orthodox medicine, judged by the same rule, stands more firmly than ever, and deserves to rank as a science, because its generalizations, as near as may be, arise out of and depend upon the greatest possible number of observed phenomena. Medicine is, however, not claimed to be perfect; wherefore, *advancement* or *progress* is not only admissible, but indispensable, and "is an index and a representative of the ultimate truth upon which the whole science is reared."

The orator next considers the benefits enjoyed by the world from our art in the *prevention of disease*, and avows himself an advocate for quarantine restrictions, "looming up amid the proudest memorials of the past;" but we are not certain that, in the case of yellow fever, no distinction ought to be made between persons and the real fomites of the disease.

The progress of medicine is succinctly stated ; and disease is said to be more successfully treated now than formerly. The duration of human life is increased, but still there is great work to be done. The greatest minds of successive ages have shed their light upon the paths wherein we tread ; and we have abundant proof that the principles of medicine are certain, real and reliable, and that the art derived from them has a substantial basis in the immutable and harmonious laws of nature.

The address is lofty in its tone, and bears evidence of much research and reflection ; and, if we might venture a criticism, it would be in respect of the undue attachment of the orator to the speculative or metaphysical, and, as consequence, to abstractions which might possibly be regarded as out of place in a class introductory. One thing is positive, however—the professor is an enthusiast in his profession ; and, as such, we wish him every success in his new position.

The lecture was published by the class.

IV.—*Lindsay & Blakiston's Physician's Visiting List, Diary and Book of Engagements, for 1861.*

THE great assistance of this publication to the medical practitioner is now admitted, and the large sale of the previous editions will certainly be an acknowledgment of the thanks of the profession to the publishers for such a convenience. The present edition contains “Dr. Marshall Hall's Ready Method in Asphyxia,” first published in 1859 ; a list of the most common poisoning substances, with the appropriate remedies ; and a table for calculating the period of utero-gestation. The arrangement is the same as that of the previous editions. The binding is neat and lasting, and the typography and paper excellent.

EDITORIAL AND MISCELLANEOUS.

THE CLOSING YEAR.

The editors of the Maryland and Virginia Medical Journal feel much gratified at its increased circulation during the last year.

We stated in the beginning, that its success would mainly depend upon those members of the profession who took an interest in its welfare, and that through it each medical philosopher amongst us could have a voice among his brethren. We also endeavored to point out some of the advantages which would flow from a medical journal circulating extensively among the profession over a circumscribed boundary. These advantages must be plain to all. The peculiarities of diseases can be more carefully studied; consultations will be facilitated; exact information can always be obtained, and whatever is of common interest to the medical profession as a body can be immediately disseminated, and unity of action secured.

In order to be alive to benefits of which we have for many years been deprived, we have only to view the subject in a physiological light, and select examples for proof in different communities. Those who are privileged to be students in the book of nature, know how one step or thought in the direction of a truth suggests another, and whenever these are quickly interchanged, progress must result. Can we doubt the agency of this fact in advancing the sciences and arts in all the large cities of the world? Our own profession would be greatly at a loss in Paris without the "Gazette Medicale," and in Vienna without the "Journal of the Imperial Society of Physicians." The same is true for the large cities in our country.*

* The multiplicity of periodicals is the evil of the day in impeding general and extensive circulation. If the two score now printed throughout this country were reduced to one-fourth of that number, properly distributed, much benefit would result.

If properly supported and conducted, the geographical position of this journal should give it an especial value for *almost* universal circulation, as within the boundaries of the two States whose names it bears, peculiarities of climate and disease are found strongly marked, which are common to both the Northern and Southern temperate zones. Relieving this, the editors spare no pains in endeavoring to extend its circulation, not only in this country, but in effecting exchanges with foreign periodicals, not without the hope that while reaping additional knowledge from such interchanges, we may sometimes be the means of imparting information to them.

With a firm determination to enlarge in all its departments the organ of the science in this section of country, and with an honest hope that their brethren will continue to regard it as their representative, the editors announce with pleasure that hereafter their department of this journal will be strengthened by the efficient aid of Professor WM. A. HAMMOND, of Baltimore. This gentleman is favorably known to the profession as an earnest and original investigator—one who has already made his mark on the literature of the science, and thus anticipate valuable assistance from this source in future.

With an unaffected sense of many shortcomings in the past, with many errors of judgment and good taste to lament, yet feeling that at least we may claim the credit of good intentions and an impartial spirit, we bring our fifteenth volume to a close, promising to give our kind friends an early New Year's call.

TO CASH SUBSCRIBERS.

As an inducement to our readers to prepay their subscriptions, we propose to furnish to all who will forward us ten dollars in advance, this Journal for the year 1861; a copy of the BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW, the best English quarterly; and also the oldest of the French monthlies, CHAMPIONNIERE'S JOURNAL OF MEDICINE, which is translated and regularly received from Paris. For this amount, the subscriber will have at his command an able representative of the science both in Great Britain and on the Continent.

SIR BENJAMIN BRODIE.

The name of Sir BENJAMIN BRODIE is so familiar to every American physician, that anything connected with his personal history will always be read with interest. For this reason, we are induced to copy the following from the London correspondent of the *British American Journal*:

“This leads me to mention that Sir B. Brodie, a few days ago, had iridectomy practised upon him by Mr. Bowman, for incipient cataract of both eyes, for the purpose of relieving any amount of pressure upon the lens, which was looked upon as likely to give rise to it. The rumors were by no means favorable as to the result, for hemorrhage took place into the anterior chamber of one eye.

“It is reported, that Mr. Lawrence, of Bartholomew’s, was astonished when he heard of it, and Sir Benjamin’s old colleague, Mr. Cæsar Hawkins, was opposed to it; and although Mr. White Cooper was present at the operation, he by no means considered that it was required. Of course, I cannot here enter into a disquisition upon the merits of this operation; like other novelties, it will have its day. until sound sense and wisdom re-enter the heads of those who just now seem to be deprived to a great extent of both.”

ERRATA.

The following typographical errors in Dr. McKew’s article in the November number require correction :

Line 4, from the commencement of the article on page 369, for “evinced,” read *enounced*.

Page 371, tenth line from bottom, erase comma after *same*, and place it after *all*.

Page 371, seventh line from bottom, for “vigorously,” read *rigorously*.

In Dr. Hammond’s article in this number (the proofs of which, we regret, were received too late for correction) the following errata appear :

Page 452, first line, for “Rummela,” read *Rummels*.

“ 453, last line, for “corrina,” read *corrina*.

“ 454, second line, for “corrignonus,” read *corregonus*.

“ 454, ninth line, for “plectiodon,” read *plestiodon*.

MEDICINE: ITS DERIDERS AND SECTS.

[The substance of the following interesting notice on the Vicissitudes and Progress of Medicine is derived from the *Revue des Deux Mondes*.]

ANTIQUITY has had its critics—either bitter and brutal, or satirical and polished. Heraclites hated Physicians; he was wont to say that they would be the most silly of men if grammarians were not there to dispute the position with them. But this morose philosopher had his own system of Medicine, and a peculiar practice founded upon his theories of Nature. He made such good use of it, indeed, that he at last died through it. Empedocles—jealous of the Physician Acron, illustrious by his writings and experience—gave himself out as a messenger from heaven, charged with the mission of exterminating diseases and other destructive scourges; he journeyed from town to town, carried in a splendid chariot, clothed in magnificent garments, and received adorations and sacrifices like a god. We know how he died, victim of his vanity, or scientific curiosity. Plato, again, did not spare the Doctors; he mocked with pleasure at their incapacity; but he, nevertheless, had a system of his own, which he had picked up from every quarter, as was his habit. From this we may conclude, that from the earliest days there has been a rivalry between doctors and philosophers, and that the last were jealous of the first.

The Greeks confined themselves to epigrams; but it was otherwise with the Romans. Physicians came at a late date into Rome, and had a difficulty in keeping their ground there. The elder Cato hated them, and prevented his son consulting them. And yet the rude censor practised Medicine in his own fashion; he possessed infallible secrets and efficacious panaceas. His method was simple enough, and, absolute master of his house, he treated man and beast alike. Pliny gives us these details, and Pliny, we know, was not favorable to doctors. In Martial's epigrams, to say nothing of other Latin poets, the doctors are ill-treated enough, and, we must admit, not without justice. The Profession was in the hands of slaves, and degraded by venal souls, easy instruments and too often accomplices of corruption, debauchery, immorality and crime. Decay had then invaded every thing.

Next came the Barbarians and universal confusion; and we lose

sight of Medicine during the first centuries of the middle ages. To the Arabs we are indebted for a sort of *Renaissance*; but it was in the first Universities that the practice of Medicine took the direction and the proper character which it bears still to this day. Now appeared the true Physician, and by his side an adversary far more formidable than his opponent of antiquity.

Before the middle age, the art of Medicine was decaying fast; and as it passed through this long period it still continued in decay. The traditions of the Greeks were gradually lost; the exercise of the art fell into the hands of monks and clergy, for the most part very ignorant, and hence, superstitious practices and absurd proceedings—the supernatural and marvellous being put in the place of experience and good sense. It was a time of miracles and prodigies—the sorcerers rivalling the saints; and while the plague and the lepra committed ravages among the people, the resources of Medicine were useless to arrest the scourges. The Jews at this time were hated and persecuted; but yet they were run after for their medical knowledge, and for the drugs obtained by them from the East through traffic with the Arabs.

The *Renaissance* awakened a spirit of inquiry. The records of antiquity, once again opened, were discoveries as of a new world. And then began the general strife against orthodoxy. Heretics and Protestants were to be found elsewhere besides in the Church. Aristotle and Galen were treated like the Pope, and so commenced the long quarrel between ancients and moderns.

This struggle, also, Medicine has passed through; but it gained an infinite number of enemies; and chiefly the Charlatans. At an early hour these industrious gentry seized upon Medicine, which offered so vast a field for the exercise of their ingenuity. From Montaigne down to Rousseau—not to mount higher or go down later—we find a concert of invectives against Medicine, the noise of them being still audible, though weak. Infinite variations were played continually on the same strain. It has taken three centuries to reduce the pretensions of physicians to their true proportions. Chemistry, which appeared at the first dawn of the *Renaissance*, explained all the phenomena of the animal economy by the principles of a gross chemistry—seeing there nothing but fermentation, distillation, and effervescence of humors at work in the living laboratory. Then later, after the discoveries of Galileo and Newton, mechanics with its levers and instruments, explained the forces; and after Harvey, hydraulics. Thus arose the

sub-sects—Iatro-chemists, Iatro-mechanicians, and Iatro-mathematicians. These were Materialists, and soon found their opponents, the Spiritualists. Truth was with neither sect; but the Spiritualists have rendered most service to Medicine. Stahl produced Barthez and Bordue, and Bordue produced Bichat. Still, even at this present day—but how enfeebled!—the two parties are face to face. Gradually they are disappearing, leaving in history the remembrance of their long and ardent struggles—prolonged for three centuries and a half—from the end of the middle ages to the commencement of the French revolution.

Two sects—either through indifference or through calculation, kept clear of the struggle—the Empirics and the Sceptics. At the head of the Empirics was Sydenham. Their business was to watch attentively the origin and progress of phenomena, noting with scrupulous care the effects of remedies and the fruit of observation—leaving aside all useless speculations.

Scepticism glided into Medicine, thanks to the demisavans. The title of Sceptic belongs to those narrowed and pretentious souls, who, contented with a superficial view, seize only the surface of things, losing sight of the links which unite them, and boldly denying the existence of whatever escapes them, affirming in this absolute negative their incapacity and deficiency. Lower still in the scale of systems we find the Eclectics—physicians who, following certain Metaphysicians, think to find a perfect system in taking what is good out of every system. Their appearance on the stage announces the end of systems. In the scientific as in the social order, end means transition, a new phasis, commencement of a new order. Medicine, which has undergone so many vicissitudes, is thus at present traversing a period of transition; it is in the way of organization, in a provisional state.

THE CHOLERA continues to make numerous victims in Spain. This mysterious scourge observes no order in its progress, attacking one province and sparing another, without anybody being able to assign a cause for its presence in one place rather than in another. It is at present at Toledo, where fifty persons have been attacked within a few days. It is to be remarked, however, that the disease is decreasing in intensity. Thus, the deaths are diminishing by nearly one-half. Or one hundred persons attacked, not more than five or six die.

VARIETIES.

GARIBALDI'S HOSPITALS.—The following extracts from a private letter to the Editor, written by a friend who has recently visited Naples, will be read with interest. The letter is dated Naples, Sept. 27, and was received this week :

“A few days ago I was one of a party which went down to Caserta to visit the military Hospital, where the wounded were brought after the unfortunate affair of Capua. We first walked in the gardens of the palace, where we were so fortunate as to come, accidentally, on a body of officers practising rifle-shooting. Among these was no less a personage than Garibaldi himself. As soon as he perceived us he came frankly forward to Mrs. W——, who had with her a large hamper of lemons and oranges for the wounded. On her requesting him to give orders that she might be admitted to the Hoepital, he immediately offered to escort her thither himself. A few minutes afterwards the people were a little astonished to see Garibaldi drive off with an English lady and her two daughters, without any of his Staff, to visit the Hospital. I was perched on the box. After a ten minutes' drive we arrived at the Hospital, a fine modern building to look on from without, but the wards are too small, and the ventilation is bad. As to organization there is none. When the crowd outside knew that the General was there, they flocked into the wards in such numbers that he could hardly move about. It was a most affecting sight to see the tenderness with which the weather-beaten warrior approached the sick-bed. Every one was visited in turn, and he had a few words of consolation for each individual. He laid his rough hand so kindly on the fevered brows of the most suffering, that the tears started from their eyes. It was a hot, sultry day, and the air was very carefully excluded. The reason given to me, on my asking why the windows were not open, was that air too freely admitted was apt to cause fever among wounded men. Of course there was no answering such a profound observation, but after a few minutes Garibaldi ordered the windows to be thrown open, Mrs. W—— having suggested to him the propriety of doing so. Before leaving the wards, where there were about sixty wounded, the most of them having been sent into Naples, Garibaldi made his secretary, who had followed him, give each patient a dollar to get any little thing he might fancy. On seeing one poor fellow very uncomfortable with a dry dressing, the usual Neapolitan one, he caused him to be properly dressed, and made some observations about using water for dressings, by which the surgeons here might profit. He asked particularly if they used chloroform during operations, and was answered in the affirmative. This is a step in advance. On the visit to the common soldiers being finished, Garibaldi went to see the officers, of whom there were about a dozen. He kissed some of them affectionately, and shook hands warmly with all, requesting every one to make

known to him any wants, etc. Not one of them complained to him, but on being questioned by Mrs. W——, it was evident that there were many things required for their comfort; above all, night attendance. The place is full of quacks and pretenders of all kinds, both Medical and military. One *gentleman*, calling himself an Inspector-General of Garibaldi's Hospitals, was offering quinine for sale the other day in Naples. Query, where did it come from? I saw a case where the hæmorrhage had been stopped by Dr. Simpson's plan. I cannot enter into details, as it is just post time, and I write in haste. One of the surgeons was present when I was showing the *modus operandi* in the Hospital here. He had tried in vain to secure the artery, a small one, and the man was getting faint from loss of blood. He then thought of the needle, and succeeded in arresting the bleeding instantly. So you see they may begin to use it by-and-by."

EFFECT OF MUSIC ON THE SICK.—The effect of music upon the sick has been scarcely at all noticed. In fact, its expensiveness, as it is now, makes any general application of it out of the question. I will only remark here, that wind instruments, including the human voice, and stringed instruments, capable of continuous sound, have generally a beneficial effect—while the piano-forte, with such instruments as have no continuity of sound, have just the reverse. The finest piano-forte playing will damage the sick, while an air like "Home, sweet Home," or "As-isa al pié d'un salice," on the most ordinary grinding-organ, will sensibly soothe them—and this is quite independent of association.—*Florence Nightingale*.

DR. FORGET ON ANTIPHLOGISTIC TREATMENT.—Broussais invented inflammation and bleeding it seems. Hippocrates, Celsus, Galen, never suspected the existence of inflammation. MM. Andral and Gavarret are only the satraps of the reformer; and as for bleeding, this same Galen, and Sydenham, Botal, Guy-Patin, Chirac, Sylva, Hecquet, Bosquillon, and *tutti quanti*, never used the lancet. Hence, then,

"Si nous soignons sans succès,
C'est la faute de Broussais."

Unable to destroy inflammation, we have begun to calumniate it; it has completely changed its nature, we are told. For 3000 years it has been considered as an *exaggeration* of the vital forces, but we have it now demonstrated as a *depression* of the nervous system. For 3000 years bleeding has been used as the most natural means of disgorgeing the inflamed tissues, and now it is irrefragably proved, that bleeding aggravates and produces inflammation. We have not only theoretical proofs of the fact, but we have practical proofs, which no one will refuse—clinical experiment. Thus English, German and French practitioners have taken it into their heads to lay the bridle on the neck of the phlegmasiæ of pneumonia and erysipelas, for example; some of them have the courage even to treat them with alcohol, by the aid of which their patients do admirably. Ah! you will

say, that does not surprise me at all; we have had expectant doctors, like Hippocrates; and busy doctors, like Aesclepiades; and debilitating doctors, like Sydenham and Broussais; and stimulating doctors, like Morton and Brown. At all times there have been empirics and methodists, who have, turn by turn, ruled the medical world. Broussais dethroned Brown, and now behold Brown revenging himself on Broussais. Science, like representative government, is a system of balancing. The truth of to-day is the error of the morrow. All which proves there is no constancy in Nature, as Richat has said, and proves also, that Nature is stronger than physic and physicians; for if she were the slave of systems, the world would soon be a desert. But statistics? you will say. Statistics are always on the side of the party who invokes their aid; they are kindly handmaids, at the service of the first comer. The value of statistics is the value of the observer. Science, good sense and honesty are of more worth than figures.

LINNÆUS' GEOGRAPHICAL CLASSIFICATION OF THE HUMAN RACE.

Homo Americanus,	{ Pertinax, contentus, liber. Regitur consuetudine.
Homo Europæus,	{ Levis, argutus, inventor. Regitur ritibus.
Homo Asiaticus,	{ Severus, fastuosus, avarus. Regitur opinionibus.
Homo Afer,	{ Vafer, segnis, negligens. Regitur arbitrio.

[*Systema Naturæ.*

"IN SCARLATINA," says Sydenham, "I hold it sufficient for the patient to abstain wholly from animal food and from fermented liquors; to keep always in-doors, and not to keep always to his bed. When the desquamation is complete, and when the symptoms are departing, I consider it proper to purge the patient with some mild laxative, accommodated to his age and strength. By treatment, thus simple and natural, this ailment—we can hardly call it more—is dispelled without either trouble or danger; whereas, if, on the other hand, we over-treat the patient by confining him to his bed, or by throwing in cordials and other superfluous and over-learned medicines, the disease is aggravated, and the sick man dies of his doctor."

"THE HISTORY OF IODINE," says Dr. Forget, "is very remarkable. First introduced as an anti-goitre remedy, it was long before it got any credit in scrofula. For some years it has had a tremendous run. In its new phasis it began by curing hydrocele; then a bold hand introduced it in hydrathrosis; then it passed into ascites, then into hydrothorax, into the pericardium, and I believe it has penetrated even into hydrocephalus. This is not all; it cures ovarian cysts, chronic abscesses, fistulæ, caries of the bones; it disinfects, deterges, and cicatrises wounds of a bad kind; it cures diphtheritis, dysentery, etc., etc. Ah! here is a remedy which men call specific!"





1 gal
155

